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THE
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Art. I.—*Yearly Statement of the progress of Physical and Chemical Science, delivered on the 31st March 1829. By* Jacob Berzelius.—*Arsberättelse om Framstegen i Physik och Chemie Afgiven den 31 Mars 1829, af Jac. Berzelius.*

The yearly statements of this eminent chemist, though generally late in reaching this country, are always acceptable to scientific men. The yearly reports of the French Academy are confined to their own labours; those drawn up by the several Professors connected with the Academy in Stockholm embrace the entire range of annual discovery. Each Professor marks every advance in his own department, and records it in his report. Thus zoology falls to the lot of Neilsen, the mechanical sciences to Pasch, and chemical physics to Berzelius. Nor are these statements mere extracts from the journals of the preceding year; they contain much valuable criticism, from which the importance of the researches of others may in some measure be estimated, and their influence on the state of knowledge more clearly comprehended. It is this latter portion of the work which gives its chief value to the reports of Berzelius. Standing in general estimation at the head of his science, much interest naturally and justly attaches to his annual criticisms. While his statements serve as landmarks, showing the precise length towhich discovery goes in each branch of the science during each successive year, they also compare former knowledge with that to which we now attain, distinguishing what is really new, and assigning to every one his due merit,—and, holding the balance between conflicting opinions, concerning which experiment pronounces nothing decisive, endeavour to point out what may be the true state of things by the deductions of a wide analogy.

The present report, as the title bears, was delivered on the last day of March 1829, but, owing to circumstances which kept back the other reports, the volume of which it forms a part was not published till the month of October. It is generally translated into the German within two or three months after its appearance, and often into French perhaps as many later; but, excepting occasional extracts taken chiefly from foreign journals, little has appeared in our language regarding the yearly statements of Berzelius. An analysis of this work will, therefore, we conceive be highly interesting to our readers, and we shall endeavour to make it more so, by confining our attention chiefly to the interspersed remarks of the author, and to experimental results which have not been already recorded in this *Journal*. The report embraces first Physics and Inorganic Chemistry, next Mineralogy, to which succeed Vegetable and Animal Chemistry. We shall follow him through these several departments.

After some notices regarding sound and light, he comes to the subject of electricity, on the phenomena and theory of which so many able men are at present engaged. Of Mr Ritchie's experiment on iron raised to a white heat, showing that a ball at this temperature has the same dispersive power long known to belong to metallic points, he observes, "Ritchie seems to have left out of view the influence of iron at a white heat on the surrounding atmosphere."

But his most important remarks are in reference to the papers of De la Rive in the 37th and 39th volumes of the *Annales de Chimie*, on the developement of electricity in the pile. In these papers De la Rive has shown, by a series of ingenious and admirably conducted experiments, that the electrical theory, or theory of contact advanced by Volta, cannot be the true theory of the galvanic pile; and embracing, therefore, the other, the chemical theory, he endeavours by the aid of it to explain all the phenomena he has observed, while, *vice versa*, he contends that these experiments prove the chemical theory. His investigations, in short, are not made in search of a true theory, but assuming one of the two received theories to be true, his experiments, and many of them are exceedingly interesting, and devised with much skill and judgment, are intended to show the chemical to be the true one. This latter theory, indeed, is the simpler, and that which has the easiest and more *visible* proof on its side, and which, from the zeal of its advocates, is at present making the greater way. Still there are some few phenomena which it fails satisfactorily to account for, and which have led some philosophers, without rejecting the great influence of chemical action, to refuse it the sole agency in the developement of galvanic electricity. Among these is Berzelius. Though in his treatise upon the "*Theory of the Electrical Pile*," published in 1807, he advocated the chemical theory, yet later experiments have convinced him, that, however intimate may be the connection of chemical action with the phenomena of galvanism, yet that they have not their origin in this action, or at least cannot in all cases be accounted for without the co-operation of the conducting power supposed in the electric theory. To such of our readers as take an interest in these theoretical discussions, we would recommend a reference to the first part of Berzelius's Chemistry, where they are shortly discussed with the author's usual sagacity.

Among the experiments of De la Rive, Berzelius finds one equally opposed to both theories. "If a glass tube be bent in the form of a U, into the one end of which is put sulphuric and into the other nitric acid, so as to keep them unmixed, and if an arch of zinc and copper be so placed that the zinc shall be in contact with the former and the copper with the latter, no chemical action takes place in the sulphuric acid, but the copper is dissolved by the nitric; nevertheless the zinc is positive and the copper negative, contrary to what ought to follow if the chemical action were the source of the electricity. To remove the powerful objection to his opinion to which this seems to give rise, he shows that if the arch consist of one metal only, the direction of the stream is still the same, though its intensity is less. Hence, says he, it is clear that when a single metal produces a stream in the same direction as two, the contact between two, copper and zinc, cannot have been the cause of the stream; but in his eagerness to overthrow Volta's theory, De la Rive seems to have forgot that the result of the experiment controverts as strongly the opinion he wishes to support, that the chemical action is the primary cause of the electrical phenomena, since while the chemical action is in the one acid, the direction of the chemical phenomena is from the other. He endeavours to explain this on the principle, that the electrical stream finds greater difficulty in passing from the copper to the sulphuric acid, than from the latter to the nitric acid, but that is to let a slight hindrance overcome a powerful action."

But it is obvious that this experiment affords another argument still in favour of the conjoined influence of the two generally assigned causes as held by Berzelius. For if when the arch of metal is of copper alone the electricity is of small intensity, and yet without any addition to the *apparent* chemical action, becomes of greater intensity on merely replacing a portion of the copper by a wire of zinc, it is evident that the circumstance of contact has an influence, if not in the primary developement of the electricity, at least in making it sensible. Or if the action of the nitric acid upon the copper appear to increase by making the arch in part of both metals while at the same time the electrical intensity is increased, it might even be presumed that the *contact* of the two metals influences the primary developement itself of the electricity. At all events, it does seem, both from this experiment and from the phenomena of the compound piles described by Berzelius in his Lärbok, which De la Rive has endeavoured, though unsatisfactorily, to explain according to his own views, that at present we cannot entirely account for all the appearances of bodies influenced by galvanic electricity by either of the theories taken alone.

Mr Ritchie of Tain, in a paper from the *Philosophical Transactions*, inserted in this *Journal*, vol. ii. p. 150, has brought forward one or two other experiments which he conceives to be hostile to the chemical theory. While upon this subject, we may take a glance at these experiments. Experiment third is hostile to the theory, because in a combination of two zinc plates immersed in water, the addition of *nitrous* acid to the fluid surrounding one of these plates induces *negative* electricity upon that plate, instead of positive, as the theory supposes. Now of the *true* nitrous acid we know by the experiments of Gay-Lussac, that when poured into water it is decomposed, forming nitric acid, hyponitrous acid, and deutoxide of azote, in proportions varying with the extent of dilution. The greater the quantity of water the more nitric acid and deutoxide is produced, and the less hyponitrous, and *vice versa.* When nitrous acid, therefore, is poured into one of the cups, a series of chemical actions take place independent of that of the acid upon the zinc, and the result obtained by Mr Ritchie merely shows that the effect of the whole series is to produce such a change on one of the plates as to make it negative in regard to the other. When such complicated actions take place, all of them involving changes in electrical state, nothing certain can be inferred affecting the truth or falsehood of the chemical theory. As the nitrous acid tends to form much hyponitrous, when the quantity of water is small, it would be curious to see if the electricity developed in this case were the same as before; but the acid must be newly distilled as it speedily decomposes. If it have any shade of green, it already, according to Dulong, contains much nitric acid.

This to us seems a very probable solution of the difficulty, and we have been the more particular in stating it, because, though partaking in some degree of the views of Berzelius, we are desirous, if possible, to get rid of the anomalies in galvanic electricity, made known to us by the multiplicator; and because this is not the first anomaly which the action of nitrous acid upon metals has presented. So long ago as 1790, Kier found that iron, after being subjected some time to the action of a mixture of nitric and nitrous acids, lost the power of precipitating silver, which may again be restored by rubbing or filing its surface. This fact has more lately drawn the attention of Wetzlar and Fechner, who have shown it to be owing to a change of electrical state undergone by the surface of the metal, which would in itself be no anomaly, were it not that this surface continues still *quite metallic.* It is possible that internal changes in the fluid, aided by, or independent of, the introduction of the metal, may be the cause of the phenomena, as we have supposed in regard to Mr Ritchie's experiment.

This is rendered more probable by what takes place in regard to another class of apparently anomalous cases. If a combination of iron and copper be plunged into a solution of a sulphur salt, as sulphuret of potash, the copper is at first negative, but after a short time becomes positive. Davy attributed this to the formation of a new solid body on the surface of the metal, by which its electricity is changed. And if, accordingly, the copper be removed from the liquid and washed, it is restored to its former condition, is negative when again plunged into the liquid, and as speedily becomes positive. All these anomalies, though they may not at present admit of the most satisfactory explanation, tend at least to prove the same thing, that unseen and unappreciable chemical actions in the fluid employed, due sometimes to the presence of the metals, sometimes to the mixtures it contains, tend to modify, and sometimes entirely to change the nature of the electricity which is developed.

Mr Ritchie's fourth experiment is less exceptionable. The true explanation of it will probably be found in the chemical properties of tin, in relation to the acids. We know that copper and iron, with a slight coating of oxide, are negative in regard to pure copper and iron. This is probably the case also with tin. Nitric acid speedily converts tin into an oxide; it requires, therefore, only that the aqua regia employed should have an excess of nitric acid to put the plate of metal in a condition to act as a negative body,—that the nitric acid should be in quantity sufficient to form a portion of oxide on the surface. The whole plate in this case will be negative, in regard to another clean plate, and the greater surface, that is, the grooved one will be negative in regard to the plain one, as in Mr Ritchie's experiment. De la Rive has shown, that an electrical current may be established by one liquid and one plate of metal, having a smooth side and a rough one, which he brings as an argument against the theory of Volta. If Mr Ritchie has not seen this in the 37th volume of the *Annales de*

Chimie, as seems to be implied by the similarity of the introduction to his own paper, and his first experiment to some of the statements of De la Rive, he will find a reference to them exceedingly interesting.

Watkins has carried the simplicity of this combination of De la Rive still farther. He has succeeded in forming a pile of a perceptible tension, with plates of zinc alone polished on the one side, and rough on the other, without any moist conductor. They are placed in a wooden frame parallel to each other, at a distance of one or two millimetres, so as to have a thin layer of air between each. The combination is air, rough zinc, smooth zinc, air, &c. and the rough zinc is positive, as when the place of the air is supplied by a liquid. This has been explained like de Luc's pile, by the oxidizing effect of the air, but it is difficult to conceive that such arrangements can owe their polarity to any chemical action upon the metallic surfaces, since Berzelius, in commenting upon this opinion of Davy in his *Arsberättelse* for 1827, states, that he has kept a pile formed of tin and brass papers, in which the tin is the positive metal, in activity for eight years, and yet the tin paper remained to the last *as pure and brilliant as when first employed.* And that the moisture of the paper which was supposed to be instrumental in causing oxidation has no connection with the electro-motive agency, has been long ago shown by the experiments of Jager, in whose piles the papers were dried and sealed up with a non-conducting body, at the temperature of 140 Fahr., so as to be air-tight, and yet continued in activity. These phenomena cannot be explained without having recourse to something more than chemical action, "which seems by no means necessary for the developement of the electrical elements in the dry pile. The absence of chemical phenomena may be remarked by the small quantity of electricity and the great tension, and their presence by the great quantity of electricity, and the small tension in proportion to it. Independent of chemical phenomena, they may possibly depend upon the unequal conducting power of the metals employed, and their consequent unequal capacity for electricity in its distributed or polarized state, which must be greater in good than in bad conductors."

The phenomena detailed under experiment fifth were unexpected, but we do not see that they are on that account hostile to the chemical theory any more than the similar facts, that a polished or rolled plate of zinc is negative in regard to another of the same metal, which is cast and unpolished. From these facts we should have expected, as Mr Ritchie seems to have done, that the hammered plate would be negative also. But it is positive, owing to the change of structure induced by hammering probably its being rendered more dense, so as to present more points to the action of the acid. Had Mr Ritchie shown, that, while hammering rendered the plate positive, it rendered it at the same time less assailable by the acid, then he might have set it up against the chemical theory. That the hardening of steel, again, renders it negative is simply another fact, valuable as such, but having no claim, that we can perceive, to a place among facts hostile to a theory which they in reality confirm.

The apparent anomaly in experiment sixth is probably owing in a great measure to the action of the hot iron upon the water, evaporation of which is generally connected with the developement of electricity. * Or it may be that by heating, a thin coating of oxide is formed upon the iron previous to plunging into the water, which, from the experiments of De la Rive, is sufficient to render it negative.

We cannot at all agree with Mr Ritchie in the conclusions he draws from his seventh experiment: "Since, he says, the zinc is dissolved without the assistance of oxygen from the water, it appears that the atoms of the acid have combined with the pure brilliant atoms of the metal, without the necessity of the metal being first converted to an oxide." Now, if the zinc, as Mr Ritchie states, was dissolved by the dilute sulphuric acid, without the evolution of hydrogen, which it would require repetition in a more delicate apparatus, as well as a

---

* According to Becquerel and Pouillet, this is not the case with perfectly pure water, but a minute portion of foreign matter in solution causes the evolution, now of the one, and now of the other electricity.—Since writing the above, we have conversed on the subject with Mr Kemp of Edinburgh, who informs us that he has made this experiment long ago with *a hot plate*, and has always accounted for it by evaporation.

subsequent analysis of the solution to establish, still it would only show, that sulphuric acid, under certain circumstances, gives up an atom of its oxygen to metallic zinc, and forms with it afterwards a *sulphite.* According to this view, the results are no way inconsistent with the chemical theory, nor "with any of the generally received notions of chemists."

But the part of De la Rive's papers which calls forth the chief remarks of Berzelius, is that in which he draws from his experiments conclusions opposed to the truth of the electro-chemical theory of affinity. We shall give an outline of the experiments and deductions, and subjoin the observations of Berzelius.

First, He shows that one metal and one liquid may produce a current, if one surface of the metal be smooth and the other rough or scratched.

Second, That if a galvanic pair of copper and tin be dipped into caustic ammonia, the copper is positive and the tin negative, while if it be dipped into caustic potash, or a dilute acid, the tin is positive, as we might expect, *a priori,* and the copper negative. The reason is, that in the ammonia the copper only is dissolved and not the tin. While in the potash ley, and diluted acid, the tin only is dissolved. This, says Berzelius, is in my opinion the most interesting experiment he has brought forward, since ammonia and potash are liquids of the same kind, which electro-chemically ought to act upon metals in the same way.

Third, Another well devised experiment consisted in plunging a small combination of lead and copper alternately in dilute and concentrated nitric acid. In the concentrated acid the copper was dissolved, and was therefore positive,—in the dilute acid the lead, on the contrary, was acted upon, and the copper was negative. The result of many experiments is embodied in the following series, in which each metal is positive in regard to all those above it:

Concentrated nitric acid.	Dilute nitric acid.
Iron oxidized.	Silver.
Silver.	Copper.
Mercury.	Iron oxidized.
Lead.	Iron.

Copper.	Lead.
Iron.	Mercury.
Zinc.	Tin.
Tin.	Zinc.

The dissimilarities in these series are all so connected with the action of the acid on the different metals, that the developement of the electricity plainly flows from the chemical action, and not from the contact of the two metals, for other circumstances remaining the same, increase or decrease in the quantity of water present, cannot; according to Volta's theory, be supposed to change the polarity. In like manner charcoal and platina in aqua regia give the platina positive, and in sulphuric acid of 100 or 150 the charcoal positive, according always as the one or the other is acted upon. Iron also is positive with arsenic in a dilute acid, but negative in fused caustic potash, because the iron is dissolved by the acid and the arsenic by the alkali.

From these experiments, besides other deductions which we pass over, he draws the following conclusions affecting the electro-chemical theory:—

"In this theory, (the chemical theory of galvanism,) the electric state of a hydro-electric pair is not derived from an electric principle belonging to each separate body in a way peculiar to itself, as is commonly understood when we say that zinc is positive in respect of copper, or that an acid is strongly negative, an alkali strongly positive; but the electricity is derived from the action of a chemical agent on the surface of a solid body. It is this which separates the two electricities in a manner analogous to friction, percussion, and all the mechanical actions which excite motion among a greater or less number of the minute parts of a body. If this be the case, and if it be correct that contact of itself cannot liberate free electricity, we may conclude that the developement of electricity cannot take place unless some kind of action give rise to it."

"Can the electro-chemical theory, in which it is set forth that the affinities by virtue of which bodies strive to unite, are nothing else than the result of their opposite electrical state? Can this theory be reconciled with the experiments above detailed, especially with the fact, that a body may be positive in respect of one body and negative in respect of another?"

"This theory seems to me to depend chiefly upon two facts; namely, the one, that bodies have a particular electricity which contact developes,—a fact, the erroneous nature of which I have endeavoured to show; the other, that in every decomposition caused by the electric pile, some bodies, those called negative, go to the + pole, others, those called positive, to the — pole; but I have in the foregoing treatise shown that it is not in virtue of electric tension, or as a consequence of common electric attractions and repulsions, that the decomposition takes place, since it goes on the easier the greater the conducting power of the liquid,* and the less consequently the tension. It seems to me, then, that, as we cannot admit the two facts above named, the electro-chemical theory, which depends upon them, has no sure foundation."

Berzelius's remarks on these observations are as follows:—"De la Rive seems to be persuaded that the result of his experiments overturns the application of electrical views in chemistry. I do not partake in his persuasion,—on the contrary, I think his experiments give a new confirmation to it. At the same time, all depends upon the point of view from which we set out. De la Rive treats separately of contact, chemical action, and the developement of electricity, as altogether independent of each other; and he has in one place said that chemical processes excite electricity like friction, &c. If it be right in scientific philosophy to consider these as independent, then may De la Rive's result be opposed in some measure to the electro-chemical theory. But if, on the other hand, it is right to admit, that, upon every occasion of contact between bodies, an affinity (föreningsbegär, *desire of union,*) belonging to matter begins to manifest itself,—if this affinity coincides with electric energy in such a way, that where the one shows itself the other is manifested to such a degree, that the electro-chemical theory sets them down as identical, (and this mode of considering the phenomena in connection is probably the right one,) then De la Rive's experiments contain nothing in opposition to its views. All the inversions of contact electricity which De la Rive has brought forward in his first treatise, and which he so skilfully contrived for bringing out the result he wished to arrive at, cease to be exceptions, when we remember that liquids give electricity of contact with one another as well as with solid bodies, and that this may be much stronger [*] than that between two solid bodies:—and when we remember that in the cases brought forward by De la Rive, the metals whose polarity changed in respect of each other, lie very near in the electrical series; [†] and thus their electricity is easily destroyed by a change produced by the action of the liquids upon each other or upon one or both the metals. If the electro-chemical theory be well founded, no electricity of contact can be developed in a hydro-electric combination, nor any electric stream arise unless a chemical action take place. De la Rive has shown that such is the case, and yet he has drawn from it the very opposite result, that it is opposed to the spirit of these theoretical views. So differently may facts be judged according to the point of view from which they are regarded. In general, it is proper to remember that those who would overthrow the electro-chemical theory, must not stop at those phenomena of contact electricity, in which unknown circumstances, with their inexplicable *bizarreries* often mock our efforts; but they must persuade us that the electrical state which De la Rive calls, *etat de courant*, does not overcome and destroy the strongest chemical affinities,—does not present them to us in an order quite the opposite of that in which they are previous to the action of the stream,—does not, for example, in the same liquid unite gold with chlorine, and reduce iron when the former is positive and the latter negative; and till this is done we cannot consider electrical energy and chemical affinity as two separate and independent principles."

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* Ritchie's first experiment in the paper above referred to is in no way inconsistent with this. Sulphuric acid in that case did not conduct any electricity, because, there being no chemical action, there could, according to the chemical theory, be no developement. In regard to conductors, De la Rive has shown also,—*1st*, That different liquids transmit electricity to solid bodies with different degrees of facility; *2d*, That of two metallic surfaces similar or dissimilar, that which most easily gives out its electricity to a liquid is positive in respect of the other in the same liquid; and, *3d*, That a metal or a liquid may be a good conductor for one intensity and a bad one for another.

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[*] This is exactly what we have urged above more in detail in our remarks upon Mr Ritchie's 3d experiment.

[†] This argument is still stronger against a change of polarity in the case of two plates of the same metal.

Heat.—Passing on to the subject of heat, we have an account of some researches of Svanberg into the heat of the planetary space. It is known that Fourier, in his valuable researches into this subject, deduced from the laws of radiant heat that the temperature of the planetary space is — 50° Cent. = 58 Fahr., and that the earth has nearly reached its limit of cooling. Svanberg has built his researches upon a different principle, and has obtained the same result. From his letter to Berzelius on the subject, we extract the following:—" Led by these considerations, and by the many known affinities between light and heat, which are especially remarkable in the acknowledged property of solar light to develope heat in opaque and imperfectly transparent bodies, I began by supposing that the planetary space (considered as perfectly pellucid) never undergoes any change of temperature either from the action of light or of radiant caloric, and that, therefore, the capacity for elevation of temperature above what reigns in the ethereal regions, can exist only within the limits of the planetary atmosphere. Further, that the rapidity of the change of temperature at an indefinite height above the surface of the earth, is always proportional to the rapidity of the atmosphere's corresponding change of capacity to absorb light. In this way I obtained the temperature of the atmosphere, (expressed in a function of an indefinite height above the earth's surface) containing only two arbitrary constants, of which the one is also a function of the time, and is determined always by immediate observation of the given temperature at the moment on the earth's surface; the other, namely the temperature of the planetary space, is constant, even in regard to the time.

" The numerical solution presupposes accurate observations of temperature at isolated points to a considerable height above the earth's surface, which, however, are unfortunately so extremely few, that we can have recourse among newer observations to but a single one, that of Gay-Lussac, in his aeronautic expedition. It were to be wished that the same experiments were repeated particularly in the neighbourhood of the equator, where the oscillations around the mean state of the atmosphere, and consequently the prejudicial influence of accidental circumstances are less to be dreaded. In the meantime, availing myself of this observation, I have obtained for the planetary space a temperature of — 49.85 Cent. which differs only by $\frac{1}{7}$ of a degree from the result of Fourier, deduced from the laws of heat radiated from the mass of the earth, the temperature of which he supposed to have reached its asymptotic state of absolute unchangeableness on the whole. Without believing in the identity of light and heat, or in the certainty of our photometric knowledge, I have thought it not entirely void of interest to see what result, in relation to this point, could be obtained from Lambert's statements, in regard to the absorption which takes place in a ray of light passing from the zenith through the whole atmosphere, calculated on the supposition that the differential of the increase of temperature is always proportional to that of the so absorbed light. By this process I have obtained for the required temperature — 50°35. I was most agreeably surprised by so remarkable an agreement between both of these results and that which Fourier derived from principles so different; and it affords an additional reason why the function I have given for the temperature should be taken into due consideration. The immediate results of the same are, that the temperature diminishes with a constantly diminishing velocity, as we ascend in the atmosphere, and that even at a given height, this velocity is greater the higher the temperature at the earth's surface.

" Without having in view any examination of the formulæ for determining heights by the barometer, I have, in the application of them to the observations of Guy-Lussac, shown, that in the determination of heights so uncommon as that of Gay-Lussac, causes of error may intervene, which in the case of lower and more common heights it is not necessary to take into account. To me that function is of importance, since from it I have derived a function for the refracting power of the atmosphere at all points of the trajectory of light, and I have, by way of preliminary, treated in considerable detail the formulæ derived from it, for the definitive determination of the refraction itself, in which I have proceeded so far that I have at last commenced the purely mathematical investigation of the required problem, such as it becomes after the strictest discussion of all the physical points connected with it."

Inorganic Chemistry.—Of the more important matters under this head, we shall give a brief catalogue Raisonnée.

Phosphurretted hydrogen.—Rose has shown in regard to this gas, that it throws down only those metals which phosphorus alone precipitates,—that both the hydrogen and phosphorus are oxidized at the expence of the metallic oxide,—and that the precipitates are always, except in the case of mercury, which comports itself in a peculiar manner, in the metallic state.*

Hydrate of Bromine.—Lowig has found that bromine combines with water at the freezing point, forming beautiful red octohedral crystals, which are decomposed by a heat of 54° Fahr. It may also be formed by passing gaseous bromine through a moist tube at a temperature of 30° or 40° Fahr. †

Iodide of Azote.—To prepare this compound, Mitscherlich dissolves iodine in aqua regia, and obtains chloride of iodine. This chloride saturated with ammonia gives chloride of ammonium and iodide of azote, which, if the quantity of iodine employed do not exceed a grain, may be safely collected on a small filter. ‡

Iodides of Carbon.—According to the analysis of Serullas, the solid iodide consists of 1 carbon + 3 iodine = $C I^3$, and the liquid iodide of 1 carbon + 2 iodine = $C I^2$. §

Hydriodic Acid.—Felix d'Arcet evaporates hypophosphorous acid till it begins to give off phosphuretted hydrogen,—he then mixes it with an equal weight of iodine, and applies a gentle heat. The phosphorus is acidified at the expence of the water, and the hydriodic acid comes over with such rapidity that it may be collected over mercury, and when the flask is full, be removed and corked without undergoing decomposition. ||

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* Poggendorf's *Annals*, xiv. 183.

† Ibid. p. 114.

‡ Ibid. p. 539.

§ *Ann. de Chim. et de Ph.* xxxix. p. 230.

|| Ibid. xxxvii. 220.

Chlorides of Cyanogen.—When cyanide of mercury with a little water is exposed to the sun's rays in an atmosphere of chlorine, an oily compound is formed, which collects on the bottom of the vessel. By the experiments of Serullas this is a sesqui-chloride of cyanogen = cy.^{2} chl.^{3}

2° If 20 grains of anhydrous hydrocyanic acid be exposed to the sun's rays in a flask containing 80 or 100 cubic inches of chlorine, at first a liquid and afterwards a solid partly crystallized white substance is deposited on the inside of the glass. After a few days the flask is opened, the muriatic acid blown out, and the white substance detached by a little water and fragments of glass. It is pressed, dried perfectly, and distilled. A colourless liquid passes over, which solidifies into a crystalline mass. This is bichloride of cyanogen, = cy. chl.^{2} and consists of 73.46 chlorine + 26.54 cyanogen, or 1 vol. cyan. + 2 vol. chlorine. It is white, crystallizes in needles, has a powerful odour, causing tears, and a weak pungent taste, reminding one of the smell. It has a specific gravity of 1.32, melts at 284° Fahr. and boils at 374°. It is sparingly soluble in cold, but more easily in warm water, which decomposes it, forming muriatic and cyanic acids. The cyanic acid is different from Wohler's. It dissolves with ease in alcohol and ether, and the alcoholic solution is precipitated by water. It is very poisonous.

Cyanic and Cyanous Acids.—If the bichloride last mentioned be treated with water till completely dissolved, then evaporated slowly to dryness, and the solid substance which remains purified by one or two recrystallizations, we obtain a pure colourless acid in small transparent rhombs, of a specific gravity between 1.7 and 1.8. According to Serullas, it consists of 61.89 cyanogen + 38.11 oxygen, or 1 atom cyan. + 2 atoms oxygen. The acid of Wohler has only half the oxygen, and must therefore be called the cyanous acid. Their composition is

(Serullas) Cyanic Acid, 1 atom cyan. + 2 atoms oxygen.
(Wohler's) Cyanous Acid, 1 —— cyan. + 1 —— oxygen.

The cyanic acid sublimes in needles, undergoing also a partial decomposition by the heat. It dissolves with difficulty in water, and has therefore no particular taste. It reddens litmus paper, dissolves without change in sulphuric acid, and may be thrown down by water. It is dissolved by boiling nitric acid, and separates from it by crystallization. The nitric acid may be distilled from it without producing decomposition. It does not seem to be poisonous. With potash, it gives a neutral salt, which is easily soluble, and an acid salt, which dissolves with difficulty.

Sulphuret of Cyanogen.—Lassaigne,[*] by mixing perchloride of sulphur with twice its weight of cyanide of mercury, and leaving them some days in a flask, has obtained a colourless crystalline sublimate, which is a new compound of sulphur and cyanogen, having the following properties: It sublimes spontaneously into rhomboidal scales, which powerfully decompose light. It is very volatile, has a pungent odour, and seems to act with considerable energy on the animal economy. Exposed to air and light, it soon becomes yellow. It dissolves both in water and alcohol. Its solution in water reddens litmus, and gives a red colour to the salts of iron. This is the only compound of these two elements which has hitherto been obtained in an isolated state. It contains 24 per cent. of sulphur, and may therefore be considered as a di-sulphuret of cyanogen; we have therefore three known compounds of sulphur and cyanogen,

1° 1 atom sulphur + 2 atoms cyanogen
2° 2 —— sulphur + 1 —— cyanogen
3° 4 —— sulphur + 1 —— cyanogen

The first is the sulphuret of cyanogen of Lassaigne.

The second the base of the sulpho-cyanic acid of Porret.

The third is the base of the sulphuretted sulpho-cyanic acid of Berzelius, a yellow compound obtained by heating sulphur in prussic acid vapour.

Magnesium.—Bussy [†] has obtained this metal in the form of a brown metallic powder, not oxidizing by contact with air, water, or dilute nitric acid, but dissolving in caustic potash and muriatic acid, and at a high temperature burning with residue of magnesia. He prepares it by passing potassium in vapour over dry chloride of magnesium.

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[*] Poggendorf's *Annals*, xiv. 532.

[†] *Journal de Chim. Medicale*, iv. 456.

We subjoin the following remarks of Berzelius suggested by the name of this metal: "The radicals of magnesia and manganese have their name from the same word. The one is called magnesia alba, and the other magnesia nigra. When the metal of the latter was obtained it was immediately distinguished by the name Manganesium. Philologists found this word too long, as well as badly derived, and Buttmann chose the name Manganium, from the common root of both, μαγγανον, which was adopted by Klaproth, and followed in the Swedish, German, and Danish nomenclatures. In England they retain the word manganesium. To prevent confusion, Davy adopted for the radical of magnesia the name magnium. This may be good enough in the English nomenclature, in which they are not scrupulous about regular derivations, but when distinguished German authors use both magnium and manganium, it is to introduce changes without reason, and to destroy the simple uniformity of name, which might be attained by the general use of the term manganium." This complaint of Berzelius is not without good ground. In this country we can hardly mistake between the generally received terms magnesium and manganese; but when in Germany we find magnium, magnesium, and talcium, as synonyms for the former, and mangan, manganium, and magnesium for the latter, there is at least a confusion in sound which is perplexing in a chemical nomenclature.

Glucinum and Yttrium.—We have not space for more than a notice of the reduction of these two metals by the indefatigable Wohler. He obtains them from the anhydrous chlorides by means of potassium, in a way analogous to that in which he formerly reduced aluminium. They are procured in the form of dark grey powders, not oxidized by air or water, but burning when heated, and leaving glucina and Yttria respectively. Their compounds are described in Poggendorf's *Annals*, xiii. pp. 577 and 581.

Reduction of sulphuret of Arsenic.—Liebeg has perfected the detection of sulphuret of arsenic. He draws out a fine tube, as for the reduction of arsenious acid by charcoal, drops into it a minute fragment of the sulphuret, and covers it two or three lines deep with carbonized acetate of lime. Immediately on heating, the arsenic is deposited in the metallic state on the upper portion of the tube.

Iodide of Arsenic.—Plisson has shown that the easiest way of preparing this compound is to digest three parts of metallic arsenic in fine powder, with 10 of iodine, and 100 water. When it ceases to smell of iodine it is evaporated, and the compound shoots out into red crystals consisting of

1 atom arsenic,
3 atoms iodine.

New Chromic Acid.—This green acid, described by Koechlin in the *Bulletin de Sciences Mathem. &c.* February 1828, p. 132, is shown by Berzelius to be a bitartrate, which, like many other tartrates, is not precipitated by alkalies, but gives with them crystallized double salts.

Metals.—In a former Number of this *Journal* we gave the results of several analysis of platina ores by Berzelius. The very elaborate paper in the *Transactions of the Swedish Academy* for 1828, from which these were taken, contains many valuable researches into the nature and combinations of the metals which accompany platinum. As these, we believe, have not appeared in any English Journal, we shall here present a short view of his results.

Atomic Weights.—These weights, determined from the decomposition of the double salts, differ considerably from his former numbers, and are as follows:

Platina and iridium,		1233.427
Osmium,	-	1244. 22
Rhodium,	-	651. 38
Palladium,	-	665.784*

Oxides.—Iridium has four oxides.

The protoxide = 1 iridium + 1 oxygen.
Sesqui-oxide = 2 ——— + 3 ———
Deutoxide = 1 ——— + 2 ———
Tritoxide = 1 ——— + 3 ———

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* These atomic weights, as formerly given by Dr Thomson, are rhodium 5.5, iridium 3.75, palladium 7.

The first, second, and fourth can be obtained in a separate state; the third exists only in the form of salts. The second is the most fixed. It bears a red heat without decomposition, but is decomposed by hydrogen gas at common temperatures. The blue oxide is a combination of the proto and sesqui-oxides, as the blue oxides of Molybdenum and Tungsten are combinations of two degrees of oxidation.

Osmium has a specific gravity of about ten, and forms five oxides,—

Protoxide = 1 osmium + 1 oxygen.
Sesqui-oxide = 2 ——— + 3 ———
Deutoxide = 1 ——— + 2 ———
Tritoxide = 1 ——— + 3 ———
Osmic acid = 1 ——— + 4 ———

These oxides may be obtained in a permanent form from the corresponding chlorides. The acid alone is volatile, and in close vessels the other oxides are not reduced by heat into the volatile acid and metallic osmium, as might have been expected. In open vessels they absorb oxygen, and are driven off. Osmium gives a blue oxide, a combination of the two lowest, which is obtained when a solution of the acid is mixed with sulphurous acid, and left for some time in a corked flask. The solution has exactly the appearance of a sulphate of indigo.

Rhodium has two oxides,—

Protoxide = 1 rhodium + 1 oxygen.
Tritoxide = 1 ——— + 3 ———

These combine in various proportions, but no distinct deutoxide has been obtained.

Palladium has two oxides,—

Protoxide = 1 palladium + 1 oxygen.
Deutoxide = 1 ——— + 2 ———

The second of these is new, as is the bichloride to be afterwards mentioned, and is obtained from the double salt of potash and bichloride, by digestion with caustic or carbonated alkalies. It is of a brown colour; dissolves slowly in oxygen acids, giving a golden yellow solution. From dilute muriatic acid it developes chlorine, but with concentrated forms bichloride.

Carburets.—Carbon and iridium combine with great ease, forming a quadri-carburet of iridium.

Sulphurets of iridium are obtained on decomposing the chlorides by sulphuretted hydrogen. They dissolve with ease in nitric acid, forming sulpho-salts. By a red heat they are changed into subsulphates.

Osmium has as many sulphurets as oxides, which may be formed either by the humid or the dry way. Those with less sulphur obtained by precipitation are yellow and partly soluble in water; that with most is thrown down completely from an acid solution. Heated in vacuo it gives off sulphur, and changes with combustion (eldphenomen) into a compound of two degrees of sulphurization, which contains two atoms metal with five of sulphur, and is decomposed with great difficulty by heating in hydrogen gas.

Phosphurets.—Iridium and osmium combine also with phosphorus. The sulphuret of iridium is gray, that of osmium resembles the metal, but is not volatile; leaving when heated a subphosphate of osmium.

Salts.—We have not space to give even an analysis of the interesting matter Berzelius has brought together in regard to the saline combinations of these metals. We shall merely insert, therefore, a view of the composition of the double salts.

Iridium unites with chlorine in four proportions, forming a chloride, a sesqui-chloride, a bichloride, and a terchloride, analogous to the oxides. Berzelius has formed and analyzed a series of double salts constituted as follows:—

1 atom chloride of potassium + 1 atom chloride of iridium.
1 ———— ammonium + 1 ————————
1 ———— potassium + 1 —— sesqui chloride of ———
1 ———————— + 1 —— bichloride ————
1 ———— of ammonium + 1 ————————
1 ———— of sodium + 1 ———— of iridium + 6 water.
1 ———— potassium + 1 terchloride of iridium.

The fourth of these is the common double salt formerly known.

Osmium combines with chlorine in the same proportions as iridium, and forms double salts of the same appearance and composition as those above-mentioned. Those containing potash Berzelius has formed and described.

The red double salts of rhodium, with potassium and sodium, are unlike in composition. They are composed of

3 chloride of sodium + 1 terchloride of rhodium + 18 water.
2 ——— potassium + 1 ——— + 1 ———

Of palladium Berzelius has formed two new double salts, consisting of

1 atom chloride of potassium + 1 bichloride of palladium.
1 —— ammonia + 1 chloride ———

The latter singular compound is obtained by treating chloride of palladium with caustic ammonia, and evaporating.

From these results it will be seen how much Berzelius has done for this hitherto obscure portion of chemical science, by his examination of these rare metals. The liberality of the Russian government put it in his power to prepare them in large quantities, and to subject them to repeated investigations. It was not without considerable pleasure that we saw in his laboratory a whole phial filled with metallic osmium, and not without still more that we were presented with a portion of it for our own use. But the quantity of the ore found in Russia, and the employment of platinum in coinage, will soon make the associated metals more *plentiful*.

Fulminating Silver.—Mitscherlich's mode of preparing this substance is very simple. He dissolves a silver salt in caustic ammonia to saturation, and adds caustic potash in excess. The fulminating silver falls immediately, and more is obtained by heating till the ammonia is driven off.*

Red Lead.—This substance is generally supposed to be a mixture of protoxide and peroxide of lead in variable proportions. "In some very beautiful red lead," says Dr Thomson, "I have found the proportion of protoxide amount to nearly one-half of the whole weight. Acetic acid dissolves out the protoxide, and leaves the peroxide untouched." † Fischer has shown that red lead dissolves in *concentrated* acetic acid, giving a clear colourless solution, which in a close vessel undergoes no change. Water decomposes it, and throws down the brown [*] oxide. "I have repeated this experiment," says Berzelius, "and find that a small quantity of acid converts the minium into a colourless salt; a larger quantity dissolves it. Heat throws down the brown oxide without previous dilution. This seems to show that red lead is not, as some have supposed, a compound of two oxides." If so, it will be the sesqui-oxide of lead.

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* Poggend. *Ann.* xii. p. 143.

† *First Principles*, i. 397.

Sulphurets of Lead.—Bredberg,[†] in a paper which has been thought worthy of one of the annual prizes of the Swedish Academy, has shown that lead has two lower degrees of sulphurization than those formerly known. One of these is obtained when twenty-five parts of sulphuret of lead (atom to atom,) are mixed with 21.6 of lead in fine grains, and melted for fifteen minutes under glass of borax. Its fracture is crystalline and scaly, and it is so soft, that it can be beat out under the hammer without breaking. It is a *di*-sulphuret of lead. The other is obtained by melting the same mixture in the open air without glass of borax. Its fracture is fine-grained; it is still more malleable than the former, and is also sectile. It contains only half the sulphur of the former. He has shown also, that the mixed ores known in Germany by the names of stein, bleistein, kupferrohstein, dunnstein, &c. contain the proto-sulphuret of iron combined in exact chemical proportions with the former of these, the *di*-sulphuret, with the di-sulphuret of copper, and, occasionally a proportion of zinc.

The known sulphurets of lead therefore are,—

The protosulphuret = 1 atom sulphur + 1 atom lead, the common galena.
Di-sulphuret = 1 ———— + 2 ————
Tetrasulphuret = 1 ———— + 4 ————

Bredberg is a young Swedish chemist of considerable promise, and one of the Professors in the School of Mines at Fahlun.

Isomorphism.—Mitscherlich,[‡] who has done so much to this very interesting department of crystallography, has shown that the sulphuric, selenic, and chromic acids are isomorphous, giving salts of the same form where the bases and the quantity of water is the same.

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[*] *Jahrbuch fur Ch. und Ph.* ii. p. 124.

[†] *R. Vet. Acad. Hand.* 1828, p. 126.

[‡] Poggendorff's *Annals*, xii. p. 137.

Hypophosphites.—Rose[*] has investigated these salts and their acid. His mode of preparing them is, with some slight differences, the same as that of Dulong, the discoverer. He describes them, like Dulong, to be all soluble in water. Most of them also crystallize. Of the alkaline salts that of soda crystallizes; those of potash and ammonia are deliquescent; those of barytes, strontian, and lime, crystallize, giving flexible crystals. They contain three atoms water to two of salt. That of barytes dried in the vacuum of an air-pump contains three atoms water to one of salt. The salt of magnesia crystallizes in regular octohaedrons, and contains eight atoms water. The cobalt salt forms large red regular octohaedrons, containing eight atoms water. The salt of nickel is isomorphous with the last two, and contains the same quantity of water, but does not crystallize so readily. That of lead differs from the rest in being insoluble in alcohol, and but sparingly in water, from which alcohol throws it down. The neutral salt forms crystalline plates. The acid forms also double salts. That of cobalt and lime gives red octahaedrons, containing three atoms water. They are merely a joint crystallization of the isomorphous salts, with a difference in the quantity of water. The acid is composed of

2 atoms phosphorus + 1 atom oxygen.

With protoxides it unites atom to atom. With tritoxides it forms *sesqui* salts.

Alum.—Kralowanszky has formed a lithia alum, having precisely the same composition as those already known. Fischer has also discovered a chrome alum, in which the green oxide of chrome takes the place of the alumina in the other alums. Their composition will be easily seen from the following formulæ:—

$L\,S + A\,l\,S^3 + 24$ water. Lithia alum.
$P\,S + C\,r\,S^3 + 24$[†] —— Chrome alum.

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[*] Poggendorff's *Annals*, xii. 411.

[†] Dr Thomson assigns 25 atoms water to all these alums.—*First Principles*, ii. p. 440.

It appears, therefore, that lithia is isomorphous with the other alkalies, and the tritoxide of chrome with alumina, which is also a tritoxide. Hence chromium and aluminum are probably isomorphous also.

Bromides.—The bromides of arsenic, antimony, and bismuth, consist, according to the experiments of Serullas,* of one atom bromine + three atoms metal.

Carbonate and Acetate of Copper.—Gay-Lussac† has shown that the black powder obtained from the carbonate, and the brown from the acetate, by boiling in water, are only oxide of copper. Berzelius had some years ago shown this in regard to the acetate.

Chemical Analysis.—Under this head we shall mention two particulars.

A mixture of five parts carbonate of potash, and four carbonate of soda, melt easier than either separately. Mitscherlich ‡ has applied this to the decomposition of mineral substances, which takes place with such ease in this mixture, that as much as 220 grains may be melted over the flame of a lamp. Sand thrown into it in small portions dissolves immediately with evaporation, exactly as when an acid is poured upon an alkali.

It has hitherto been a very difficult matter to determine the amount of boracic acid in mineral bodies. Dumenil dissolves the mineral in nitric acid, separates the silica by evaporation, and, with the neutral solution, mixes nitrate of silver in excess, and evaporates to dryness. On re-solution the borate of silver remains, being a di-sesqui-borate, consisting of two atoms acid + three oxide of silver. As the borate dissolves in free nitric acid, it may in this way be separated from chloride of silver. §

Mineralogy.—Of the new minerals described, and analysis given by Berzelius, several have already appeared in this *Journal.* We shall notice those only which have not previously been laid before our readers.

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* *Annal. de Chim.* xxxviii. 318.

† *An. de Chim.* xxxvii. 385.

‡ Pog. *An.* xiv. 189.

§ *Jahrbuch fur Chim. und Phys.* 182, i. 364.

Seleniurets.—Del Rio has analyzed a grayish black crystallized mineral occurring with native quicksilver in limestone, which lies on red sandstone in the mining district El Doctor-Culebras, Mexico. It consists of

Selenium 49, zinc 24, mercury 19, sulphur 1.5; which Berzelius calculates to be 4 zinc, 1 mercury, and 7 selenium, from which he deduces the formula, $2\,Zn^2\,Se^3 + Hg\,Se$; or it is a compound of

2 atoms sesqui-seleniuret of zinc + 1 seleniuret of mercury.

Kersten has examined another Mexican mineral occurring with mercury and sulphur in quartz and calcspar, and has found it to be a mixture of sulphuret and seleniuret of mercury. The mineral which Tiemann found in the Hartz, and, from its being entirely volatile, judged to be native selenium, Marx has shown to be also a seleniuret of mercury.

Silver Phyllinglans.—Under this name Breethaupt has described a mineral occurring with galena at Börsön in Hungary, in gneiss. It is dark lead-gray, and consists of thin flexible scales of a specific gravity = 5.895, and metallic lustre. It is a compound of seleniurets of silver and molybdenum.

Couzeranite.—This mineral occurs in the overlying chalk around Couzeran in the Pyrenees. It is in the form of oblique rhomboidal prisms, longitudinally striated; cross fracture uneven; longitudinal, scaly, conchoidal. Colour black; lustre glassy. Scratches glass, but not quartz. Specific gravity = 2.69. Melts before the blowpipe, but does not dissolve in acids. Dufrenoy * has found it to consist of silica 52.85, alumina 24.25, lime 12.04, magnesia 1.46, potash 5.63, soda 3.75. Dufrenoy deduces from this, that the mineral consists of one atom of the bisilicates of potash and soda + two of the tersilicates of lime and magnesia + six silicate of alumina; but as it is rare to find the alkalies and alkaline earths combined with unequal atoms of silica, Berzelius prefers the following formula:—

$$\text{1 atom tersilicate.}\left\{\begin{array}{l}\text{Lime}\\ \text{Magnesia}\\ \text{Potash}\\ \text{Soda}\end{array}\right. + \text{2 silicate of alumina.}$$

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* *Ann. de Chim. et de Ph.* xxxviii. 280.

Pectolite occurs in the Tyrol with mesotype and mesolite, the latter of which it resembles so much, that it can with difficulty be distinguished from it without the help of analysis. It has a vitreous lustre externally, but pearly within. Specific gravity = 2.69. Kobell found it to consist of silica 51.3, lime 33.77, soda 8.26, potash 1.57, water 3.89, alumina, with a little oxide of iron, 0.9. It is composed, therefore, of

$$1 \text{ atom tersilicates. } \left\{\begin{array}{l}\text{Potash}\\ \text{Soda}\end{array}\right. + 4 \text{ bisilicate of lime} + 1 \text{ water.}$$

Berzelius remarks, that before the blowpipe he found it give a strong reaction of fluoric acid, from which he infers, that the 0.9 given as alumina, was, as in the apophyllite, fluor spar dissolved and precipitated.

Okenite, a new mineral from Disk Island, Waygat's Straits, belonging to the zeolite family. It occurs in amygdaloid. Is white, fibrous, or finely radiated. Hardness between fluor and feldspar. Specific gravity = 2.28. Melts before the blowpipe with intumescence. Is decomposed by acids, and becomes gelatinous. According to Kobell, it consists of silica 56.99, lime 26.35, water 16.65; which agrees with 1 quadrisilicate of lime + 2 water, or perhaps more properly:

1 hydrated tersilicate of alumina + 1 hydrate of silica, as quadrisilicates are quite uncommon.

Karphosiderite.—Breithaupt has given this name to a mineral from Greenland, uncrystallized, kidney-shaped, earthy, and rust coloured. From its action before the blowpipe, Harkort states it to be a hydrated subphosphate of iron.

Tautolite.—This mineral occurs in lava around Laacher-dee. It is black; full of cracks; sometimes in small crystals; gives a gray streak; has a dull glassy lustre; is friable, and has a specific gravity of 3.865. Harkort examined it by the blowpipe, and considers it to be a proto-silicate of iron with silicate of alumina, but decidedly different from olivin.

Fergusonite, from Cape Farewell in Greenland, has been analyzed by Hartwell, who found it composed of tantalic acid 47.75; yttria 41.91; protoxide of cerium 4.68; zirconia 3.02; oxide of tin 1.0; oxide of uranium 0.95; oxide of iron 1.34; loss 0.35. He has classed it with the yttrotantalite, with which it agrees in composition, except that the bases in the

Fergusonite take up twice as much tantalic acid as in the yttrotantalite, and that here the oxide of cerium and zirconia take the place of the lime. Its formula is:

$$\left.\begin{matrix}\dot{Y}^3\\ \dot{C}^3\end{matrix}\right\}\dddot{T}a \text{ mixed with } \ddot{Z}r\,\dddot{T}a$$

or a tritantalate of yttria and cerium mixed with tantalate of zirconia.

Aeschynit, brought from Mias in Ural by Menge, and analyzed by Hartwell, gave titanic acid 56; zirconia 20; peroxide of cerium 15; lime 3.8; oxide of iron 2.6; oxide of tin 0.5. Hartwell does not give this as perfectly correct, because we are at present unacquainted with any method of separating correctly the titanic acid from the zirconia. From this circumstance Berzelius has given it the name Aeschynite from *αιςχυνω*, I am ashamed.

Phosphate of Copper.—Kobell has shown that the cuivre hydraté globuliforme of Haüy is a phosphate of copper, and Bergman has shown it to be identical with that from Rheinbreitbach, analyzed by Arfvedson, whose composition is,

$$2\,\dot{C}u^5\,\overset{\cdot\cdot\cdot\cdot\cdot}{P} + 5 \text{ water.}$$

Dioptase.—A specimen of perfectly pure dioptase, analysed by Heess, has been found to contain oxide of copper 48.89; protoxide of iron 2.0; silica 36.6; water 12.29; and gives the formula $\dot{C}u^3\,\dddot{S}i^2 + 3$ water. It is therefore a di-sesquisilicate of copper with three atoms of water. This differs considerably from Vauquelin's result, which, however, was obtained from an uncrystallized specimen—a silicated malachite.

White iron Sinter.—Kersten has analyzed an earthy white or grayish yellow mineral, found in reniform masses in the old mines at Fürstenstoll at Freyberg, and found it to consist of arsenic acid 30.25; peroxide of iron 40.45; water 28.5. It is therefore a di-arseinate of iron with 12 atoms of water.

Datholite.—Two new analysis of this mineral have been made by Stromeyer and Dumenil. We shall contrast them with the previous analysis of Klaproth. Stromeyer's was from Andreasberg—Klaproth's from Arendahl.

	Klaproth.	Dumenil.	Stromeyer.	Calculation.
Silica,	36.5	38.51	37.36	37.51
Lime,	35.5	35.59	35.67	38.61
Boracic acid,	24.	21.34	19.37	18.91
Water,	4.00	4.6	5.71	4.9

The calculation supposes it to be a di-borate of lime + 3 ter-silicate of lime + 2 water.

Diallage.—We cannot withhold from our readers the results of Kohler's elaborate examination of this substance, and the philosophical deductions from them as to the composition of a great variety of mineral substances.

	Diallage metalloid from Baste.	Diallage metalloid from Salzbug.	Diallage metalloid from Tuscany.	Crystallized diallage from Baste.	Bronzite from Stempel Marburg.	Bronzite from Ullenthal Tyrol.
Silica,	53.707	51.338	53.200	53.739	57.193	56.813
Magnesia,	17.552	15.692	14.909	25.093	32.669	29.677
Lime,	17.065	18.284	19.088	4.729	1.299	2.195
Protoxide of Iron,	8.079	8.230	3.671	11.510	7.461	8.464
Magnesia,			0.380	0.233	0.349	0.616
Alumina,	2.825	4.388	2.470	1.335	0.698	2.068
Water,	1.040	2.107	1.773	3.758	0.631	0.217

When we compare these results, says Berzelius, with those of Henric Rose concerning the pyroxenes, we find a striking resemblance between them, which leads to the conclusion drawn by Köhler, that they contain the same kinds of combinations, namely, bisilicates of the isomorphous bases, lime, magnesia, and protoxides of iron and manganese—and that the alumina supplies the place of a deficient quantity of silica, as is the case with pyroxene and amphibole. To the similarity of composition, Köhler has added the resemblance in the angles of the crystals, which can be shown to exist in several species. If we endeavour to find in the composition a cause for the differences in cleavage and outward appearance between diallage and pyroxene, we shall discover it by a comparison of the analysis of both classes of minerals. In the pyroxenes, the silicate of lime is seldom represented by any other bases. They are bisilicates of lime combined with mixed silicates of magnesia, protoxide of iron and manganese. In the diallage, the silicate of magnesia predominates, from the bronzite, which seems to be a mixed crystallization of bisilicate of magnesia with small quantities of the others to diallage, which is shown to be a double bisilicate of magnesia with compensating portions of bisilicates of lime, iron, and manganese, mixtures which have their origin plainly in mere chance. Neglecting the mixed alumina, therefore, we may represent these relations by the following formula:

$$\text{Pyroxene } C\,S^2 + \left.\begin{matrix}M\\F\\Mn\end{matrix}\right\}S^2 \quad \text{diallage } M\,S^2 + \left.\begin{matrix}C\\F\\Mn\end{matrix}\right\}S^2$$

$$\text{And hypersthene or bronzite } \left.\begin{matrix}M\\C\\F\\Mn\end{matrix}\right\}S^2$$

There seems to us to be three points necessary to a philosophical comprehension of the nature and mode of formation of the vast diversity of mineral substances. These are, chemical analysis, the atomic theory, and the doctrine of isomorphous bodies. Rightly applied, their aid will unlock all the mystery which variations in external characters throws around the study of mineralogy. Analysis setting before us one true atomic compound, will class along with it hundreds of others which vary only in the nature of the impurity with which they are contaminated—while the principle of isomorphism will enable us to predicate what those impurities *must* be. Except, indeed, as the handmaid of the more exact sciences, of chemistry, crystallography, or optics, the study of minerals is not in itself a science. All their combinations are *accidental.* They are formed by the union of bodies uniting, indeed, according to fixed and natural laws, but brought within the range of their mutual affinities by *chance;* for we cannot conceive it to have been intended for any wise purpose that a mineral should be found pure in one place and impure in another. The mere study of minerals, therefore, as such, is inferior to any other branch of natural history, for the meanest plant or animal that grows or moves has a principle of life in it which lifts it out from the mass of inanimate nature, and imparts a dignity to the study of its organization which many even of the recondite investigations of chemistry are far from possessing.

Epidote Manganesifere from St Marcel, has been classed by some mineralogists among amphiboles. Cordier's analysis was not satisfactory. Hartwell finds it to consist of silica 38.47; alumina 17.65; lime 21.65; peroxide of manganese 14.08; of iron 6.6; magnesia 1.82. This result shows that the crystalline form had rightly indicated the composition of an epidote, since the lime is here exactly compensated by a small quantity of magnesia, and the alumina by the isomorphous oxides of iron and manganese, of which the latter has, for the first time, been made out with certainty as one of the elements of a double silicate, and is indeed the colouring matter in this mineral. Hartwell gives the following formula,

$$\left.\begin{matrix} C \\ M \end{matrix}\right\} S + 2 \left\{\begin{matrix} A \\ Mn \\ F \end{matrix}\right\} S$$

Steatoid.—The crystallized mineral from Snarum in Norway, described by Möller under this name, has been analyzed by Hartwell, and found to be identical with noble serpentine. It consists of silica 42.97; magnesia 41.66; peroxide of iron 2.48; alumina 0.87; water and carbonic acid 12.02.

Vegetable Chemistry.—Animal and vegetable chemistry afford to the man of science the most astonishing field of contemplation. He sees all the varieties of vegetable nature reducible to four elements, and animal products in general made up of the same number, and yet there is no end to their combinations, and none can tell the limits within which the changes of nature wrought through the aid of such scanty materials must be confined. Yet this beautiful simplicity has hitherto rendered the study of her combinations perplexing, and we have been unable often to discover such differences in atomic constitution as at all to account for the diversities in external appearance and character of many vegetable and animal substances. But our knowledge is enlarging, and, as the resources of organic analysis become greater, it is to be hoped that we shall by degrees attain, if not to a complete, at least to a much less imperfect view of the chemical constitution of organized bodies than we can yet boast of.

The new researches under the head of vegetable chemistry most deserving of attention in the volume before us, are the *Equisetic acid*, found by the industrious Braconnot in the *Equisetum fluviatile*—an acid discovered by Runge in the *Scabiosa succisa*, which he calls *green* acid—a vegetable salt basis, found by Brandes in the *Chiococca racemosa*—another vegetable alkali named *Atropin*, extracted from the *Atropa Belladonna* by Ranque and Simonin—a third, by Dana, in the root of the *Sanguinaria Canadensis*, which he calls *Sanguinarin*—a fourth, by Pelletier, from the bark of a species of *Cinchona*, called *China de Calysaya* or *China de Carthagena*—a fifth, *Salicin*, prepared by Buchner from the bark of the *Salix pentandra*—and a sixth, which is the most remarkable of them all, being deliquescent, extracted by Boussingault from a substance called *Curara*, sold in South America for poisoning hunting spears, and which, according to Humboldt, is prepared from the juice of a tree analogous to the *Strychni*. Of substances possessing neither alkaline nor acid properties, Bonastre has discovered one peculiar to the wax of *Ceroxylon andicola*, a crystalline fatty matter, which he has called *Ceroxylin*—another, in the *Conium maculatum* by Brandes and Giesecke, half-a-grain of which kills a dog with the same symptoms as *Strychnin*, and which has been called *Coniin*—a third, by Dulong d'Astofort, in the *Plumbago Europæa*, named *Plumbagin*—a fourth, *Hesperidin*, by Lebreton in unripe pomegranates—and a fifth, *Tremellin*, by Brandes in the *Tremella mesenterica*, and which bears to it the same relation as *fungin* to the mushroom tribe.

Camphor.—Libri has mentioned a curious circumstance regarding odoriferous bodies such as camphor,—that if they be exposed to a current of electricity for a considerable time, their smell diminishes, and at last disappears entirely. After a lapse of time, camphor again recovers the power of emitting odours.

Animal Chemistry.—We have only space to notice under his head the very interesting experiments of Wöhler on the artificial formation of *Urea.* It is one of those rare results which approximate the productions of art to those of nature, and lay open the possibility of imitating her in some of her elaborations, though they unfortunately afford little ground for the flights of those airy speculators who hold up the probability of our being soon able to dispense altogether with the agency of nature in the production of the animal and vegetable substances, upon which we at present depend for our existence.

If *Cyanite* * of silver be treated with a solution of sal-ammoniac, or cyanite of lead with caustic ammonia, there is formed, instead of cyanite of ammonia, a crystalline matter which has all the properties of pure urea, and which therefore *is Urea.*

The constitution of urea, according to the analysis of Prout, compared with that of one atom each of ammonia, cyanous acid, and water, is as follows:

	Urea.	Cyanite of ammonia.
Hydrogen,	2 atoms.	4 atoms.
Carbon,	1	2
Oxygen,	1	2
Azote,	1	2

From which we see that the elements in both are in the same proportion, which confirms the experiments of Wöhler. Yet urea is not a cyanite of ammonia, for stronger bases do not develope ammonia. The atoms, therefore, must have combined themselves in a different manner, so that the cyanite of ammonia, from being a compound atom of the second, has passed to the state of a compound atom of the first order. This is one of those few examples from which we can accurately and strictly deduce the law, that the same number of single atoms may combine in different ways, so as to produce bodies of very different characters and properties.

Geology.—In connection with geology, we shall advert to C. G. Gmelin's elegant examination of clinkstone. He has found that this volcanic rock is an aggregate of mesotype and felspar. He shows this in a very interesting way. He treats the mineral with muriatic acid, and separates the dissolved portion, after which the silica of the decomposed part is dissolved out by boiling with carbonate of potash. In the mesotype, a portion of the soda is compensated by potash and lime, and a part of the alumina by peroxides of iron and manganese, and in like manner, in the felspar, a part of the potash is compensated by soda and lime, and of the alumina by oxides of iron and manganese. In this investigation the water in the mesotype was found to be less than in the same substance when crystallized. The water might possibly be merely hydroscopic, as its quantity in the mineral varied from 0.633 to 3.19 per cent. It is probable that the application of this principle to other rocks might be productive of very interesting results, and might throw light upon geological formations, which we shall seek in vain from the analysis of specimens of rocks in their aggregated state.

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* This is prepared with Wöhler's acid, which we have shown above must be called the Cyanous, and its compounds, of course, Cyanites.

We have left ourselves no room for further remarks on this very interesting work. Its nature, and the ability of its execution, will appear, we think, from the various extracts which we have made. With one or two exceptions from the pen of the first chemist in Scotland, no analyses of Swedish chemical works, we believe, have appeared in our language—certainly no such account of the yearly reports of Berzelius has ever been inserted in the English journals, and for these reasons, independent of the many facts we have taken occasion to lay before our readers, we hope the present article may not prove wholly uninteresting. Such a work as this of Berzelius is a great desideratum in our own country. Could any able chemist be persuaded to devote his time to such a labour, and could he find a bookseller sufficiently public-spirited, men of science would receive a most valuable gift.

ART. II.—*Experimental Inquiries concerning the Laws of Magnetic Forces.* By D. SNOW HARRIS, Esq. of Plymouth. Communicated by a Correspondent.

THE limits of this work prevent our giving more than a brief abstract of the results detailed in this interesting article from the *Transactions of the Royal Society of Edinburgh*, vol. xi. It may be divided into two parts; *1st*, A description of an apparatus invented by the author, which affords every facility for examining the intricate phenomena of induced magnetism. Its principal parts are a graduated perpendicular bar, surmounted by a divided quadrant; near the top of the bar is fixed a wheel, of which the axis rests on friction wheels. As this wheel, when in use, never makes an entire revolution, the part which stands uppermost, when at rest, is on the circumference supplied with a slight projecting arm, which serves as an index to the quadrant. A light thread passes over this wheel, (being fastened thereto,) to one extremity of which a light magnet or piece of iron (as occasion may require) is attached, and to the other a counterpoise adjusted so as to be of equal weight thereto. The delicacy of this machine is sufficient for the most minute investigations. *2d*, The results obtained by the use of this apparatus. The quantity or force of magnetism induced in a bar of soft iron by the approach of a magnet is indicated by a piece of iron A, suspended from the wheel; beneath this is fixed the iron under experiment I, and below this a magnet M, is placed, the distance A I being constantly 0.2 inch.

TABLE I.

Distance I M.	Force induced.
1.	3.
0.8	4.
0.6	5.
0.5	6
0.4	7.5
0.3	10.
0 2	15.

Hence it follows that the magnetic developement induced in the iron *increased in an inverse simple ratio of its distance from the magnet.* The truth of this receives confirmation from the weights required to break the contact of A and I, M being now placed uppermost, and A below and in contact with I.

TABLE II.

Distance I M.	Weights in grains.	Mean weight in grains required to break contact A I.
4.	between 190 and 210	200
3.	250 and 280	265
2.	390 and 400	400
1.	a little less than 800	800

But this mode of experimenting is not so accurate as the former. The same result obtains when the iron I and the magnet are placed horizontally, and the index iron A perpendicularly.

"This law of magnetic induction is observed to proceed uniformly from the distance at which the force first becomes measurable, until the iron and magnet are very nearly approximated, but then begins to vary. Thus when they were approximated within 0.1 inch, the increments in the attractive force began to diminish. From this it would appear either that the similar and distant polarities begin to exert a sensible influence, or that a limit exists approaching saturation, beyond which the inductive effect on the iron does not proceed with the same facility as before. In either case, this limit may be supposed to vary with the power of the magnet. This was found to be the case by using successively magnets of different degrees of intensity; the induced effects being at first proportional to the power of the magnet, but the increments of force by approximation begin to diminish at a greater or less distance in proportion to the intensity of the original magnetic force. *Cæteris paribus*, the attractive force of magnets by induction at their distant poles is inversely proportional to the lengths of the iron; and, as before observed, proportional to the powers of the inductive magnets." The distances of M, I in the experiments referred to in the following table = 0.3 inch, and I, A = 0.2 inch.

TABLE III.

Position of M I vertical.		Position of M I horizontal.	
Length of iron I.	Force induced 5° = 1 gr.	Length of iron I.	Force induced 5° = 1 gr.
1.	20	3	18
1.5	14	6	9
2	10		
3.	7		

It appeared in the course of the experiments, that, whether the iron I was presented to the magnet in a lateral or longitudinal direction, still the induced force of the superior pole did not materially differ. It also became evident that when A was so nearly approximated to the magnet M as to be, *when alone*, within the sphere of its action, yet when I intervened, *it appropriated to itself the attractive power, thus intercepting the effect which M would otherwise have produced on A*, A being as it were insulated thereby. By substituting a small magnet in place of the iron A, and removing M, the effect was the same whatever was the length of the iron I, it appearing that the proximate poles of magnets by induction are of equal intensity. " As the iron I receives a magnetic developement inversely proportional to its distance from the magnet M, we may consequently, by varying this distance, alter the relative magnetic intensity of I at pleasure; and thus, by fixing a second mass of iron *i*, immediately above I at a constant distance *i* I, this mass *i* can be caused to operate on A by a sort of second induction, so that by preserving the distances A *i* and *i* I, and at the same time varying I M, we have all the conditions requisite for determining the law of the inductive influence, when the force of a magnet (as we may term) I, is made to vary, but its distance from the iron *i* preserved constant. The experiment being thus arranged, it was found, as might have been anticipated, that the second mass of iron *i* received an attractive force directly proportionate to the magnetic intensity of the mass I below. The same result was obtained when the force was varied by means of magnets instead of by induction, the forces of the magnets being to each other in a known ratio." In the following table are contained the results of experiments, the distance A *i* in the series by induction, and A I in the series by magnets, also the distance *i* I in the inductive series = 0.2 inch. The distance M I = 0.5 inch.

TABLE IV.

Force varied by induction.		Force varied by magnets.	
Magnetic intensity.	Force of induction.	Magnetic intensity.	Force of induction.
1	3	1	5.
2	6	2	10.
3	9	3	15.

Hence we may conclude that the magnetic developement in masses of iron by induction is, *cæteris paribus, directly proportionate to the power of the inductive force, and inversely as the distance*, and that the forces which magnets develope in a mass of iron at a given distance, within the limits already noticed, may be taken as a fair measure of their respective intensities. It is a well known fact, that by placing a mass of iron in contact with one pole of a magnet, an increase of force apparently obtains at the other; and under similar circumstances this has no relation to the *mass* of iron, nor to the direction, whether lateral or longitudinal, in which the iron was placed with regard to the magnet. The iron A being suspended above the extremity of magnets M, of various intensities, placed horizontally, and a bar of iron I, situated in the same plane being thereto approximated.

TABLE V.

Magnetic force constant.

Distance M I.	Decrease of attractive force.
0.6	3.
0.4	4.5
0.3	6.
0.2	9.

TABLE VI.

Magnetic force variable.

Intensities of magnets.	Decrease of attractive force.
1.	2.
2.	4.
3.	6.
4.	8.

The distance A M in Table V. being 0.6 inch, and in Table VI. 0.8 inch, constant distance M I, Table VI. at which the variable forces acted 0.2 inch. A mass of iron I, being placed horizontally with one of its extremities beneath A, and a magnet M, opposed to its opposite extremity, so as to induce magnetic developement, the distance A I, and the attractive force being noted, a similar mass of iron *i*, was opposed to the induced pole; and thus, by making the distance M I constant, and varying that of I *i*, the magnetic developement in I remained constant, whilst the distance of the iron *i* was variable, and by making the distance I *i* constant, and varying I M, we vary the magnetic developement in I, whilst the distance of *i* is constant. The distance A I is constantly 0.3 inch.

Table VII.

Magnetic force by induction constant, distance variable.

Distance I *i*.	Decrease of force of attraction.
0.6	3.
0.4	4.5
0.3	6.
0 2	9.

Table VIII.

Magnetic force of induction variable, distance constant.

Distance M I.	Magnetic developement.	Decrease of force of attraction.
0.6	1	3
0.3	2	6
0.2	3	9
0.15	4	12

The effect of the iron being proportional to the power of the magnet M, and inversely as the distance M I. "That the absolute force of attraction exerted between a magnet and a piece of iron should vary with the power of the magnet, and consequently with the force induced in the iron, *cæteris paribus*, is what might have been previously supposed; but the ratio in which this same force of attraction might be expected to vary, when the force induced in the iron is a constant quantity, whilst its distance from a magnet M is variable, the magnetism of M being either temporary or permanent, is not so apparent; nor does it appear that such a case had been previously contemplated. But we have seen, that by varying the distance between a magnet and a mass of iron M I, we can within certain limits obtain any relative intensity required; and by varying the distance between the temporary pole of the iron I and A, we can preserve the force induced in A constant. Thus if we diminish the distance M I to one-half, we double the force in I: and if the distance M I be preserved, the force in A would likewise be doubled; but, if whilst we diminish M I one-half, we double I A, then the force in A will remain as before. We may thus preserve the induced force in the iron A constant, whilst its distance from the inductive magnet I is variable, and by this arrangement it was clearly shown that the absolute force *varied with the distance, the induced force in the iron being a constant quantity.* This was not only apparent when the magnetic force was varied by induction, but was also satisfactorily shown when varied by magnets, of which the relative powers of induction were previously ascertained. Thus two magnetic bars (2 M and M) whose inductive powers were as 2: 1, were successively placed immediately under the suspended iron A, the stronger magnet 2 M being placed at double the distance from A that M was; the want of power in the weaker being compensated by its diminished distance; so that the force induced in A was in each arrangement the same; the forces, however, marked by the index were inversely proportional to the distances. We may conclude from these investigations, that the actual force exerted between a magnet and a mass of iron, is directly as the force induced in the iron, and inversely as the distance."

This case applies to the general law determined by Coulomb and others. By suspending a piece of iron A, immediately above a magnet M, we thus simultaneously vary the force induced in the iron, and the distance. Thus, the distance A M being one inch, the force indicated was 5°. On diminishing the distance to 0.5 inch, the force was = 20°.

The following table contains the results of two series of experiments made with two different magnets, showing the attractive force of a magnet and iron on each other, at various distances :—

TABLE IX.

Magnet, No. 1, North Pole.		Magnet, No. 2, South Pole.	
Distance A M.	Force of attraction.	Distance A M.	Force of attraction.
1.0 inch.	4.5	1.0 inch.	6.
0.9	5.5	0.9	7.5
0.8	7.	0.8	9.5
0.7	9.5	0.7	13.
0.6	13.	0.6	17.
0.5	18.	0.5	24.

5° attraction = 1 gr.

The same result obtains when the magnet is suspended and the iron fixed beneath it.

" In order to examine the inductive action of one magnet on another, a magnetized piece of steel I, was placed under the suspended iron A, and the attractive force at a given distance only noted. A magnetic bar M, was then placed under it, first, the similar poles, and secondly, the dissimilar poles, being opposed; having previously ascertained the force of the magnetized steel I, at each pole and made them equal, and having also equalized the poles of the magnet M, and ascertained their force. The results obtained from a series of experiments thus arranged, appeared to show satisfactorily that the forces acquired or lost by the magnet I, at its superior pole, in consequence of the inductive action, were, within certain limits, in the inverse ratio of the distance I M, between the two magnets; after which the increments and decrements began to diminish. In the following table are seen the results of a series of experiments with different magnets marked 1, 2, 3, 4.

TABLE X.

Dist.	Dissimilar poles. Attraction.				Similar poles. Repulsion.			
I M.	1	2	3	4	1	2	3	4
2.0	1.	2.	2.	4.	1.0	2.	2.	4.
1.5	1.5	2.5	2.5+	5.5	1.5	2.5	2.5+	5.5
1.0	2.	3.5	4	7.	2.0	3.5	4.	7.5
0.5	3.5	6.	7	10.	3.5	4.5	5.	11
0.3	4.	7	10		4.	5.	6.	
0.2	5	9.5	11.5		4.+	5.+	7	
0.1	8.	11.	14.5		4.5	5.5	7.5	

" The limits within which the inductive action varied according to an uniform law, would, from these experiments, appear to depend on the magnetic intensities, and on the circumstances before observed; so that the precise distance at which it becomes irregular in its action is not the same for each magnet; and, it may be further observed, that when the inductive action operates in a contrary sense to the poles of the magnets, the decrements vary at last more rapidly than the increments, supposing in the latter case the induction to operate in the same sense. Similar variations from a regular law are observable when the force of a magnet is made to vary, the distance between the two magnets remaining the same. Thus a magnet of a double force, opposed to the inferior pole of another magnet, does not *at* ALL *distances* exert an inductive influence proportionate to its power on unmagnetized iron; it appearing that cases may exist in which the intensities of the magnets become so circumstanced in relation to each other, that the inductive action no longer proceeds. The absolute attractive or repulsive force exerted between two magnets at various distances will materially depend on the operation of the inductive influence, the induced forces and the distances being both variable; for we have seen that the absolute force exerted between a magnet and a mass of iron varies with these quantities conjointly. The same may therefore be inferred of the absolute force exerted between two magnets. Thus, when only one of the bodies M, is permanently magnetic, the absolute force is directly as the force induced in A, and inversely as the distance, and this is still true when A is also a magnet, seeing that the inductive action still proceeds; and this will be still true whatever be the relative magnetic intensities, the only difference being, that in the first case there is one primary inductive action, whilst in the second there are two. Therefore, the whole attractive force between M and A will be still found to vary in an inverse ratio of the square of the distance, the inductive action being supposed to go on uniformly."

The absolute force of attraction or repulsion between two magnetized bodies may be readily determined by substituting a magnet M, in place of the iron A. It may be readily ascertained that a point may be discovered, at and beyond which the action of similar poles on one another (when opposite poles are opposed,) are of no value; and it has been seen that the inductive influence of dissimilar polarities lessens their free action. The approximation of N to S, supposing them opposite poles of different magnets, tend to neutralize each others force, and to extend the line beyond which the poles N and S exercise no influence on one another; and this may be so far extended, that the forces N and S, are reduced to two insulated points. The approximation of N and S may contract this line, yet the inductive force tends to reverse the repelling poles. The following table contains results obtained by five magnets, *a*, *b*, *c*, *d*, *e*

TABLE XI.

Dist.	Dissimilar poles.					Similar poles.				
m M	*a*	*b*	*c*	*d*	*e*	*a*	*b*	*c*	*d*	*e*
4.					3. |					3. +
3.5					4. |					4. |
3.					6. —					6. —
2.5					8.5					8. +
2.			2.5	3.	13.			2.	2.5	13.
1.8			3. +	3.5 +	16.5			2.5	3. +	15. +
1.6			4.	4.5 +	21.			3.	4. +	18.5
1.5			4.5	5.5	23.			4.	5.	20.
1.4			5.5	6. +	28.			4.5	5.5	23.
1.2			7.	8.5	38.			5.5	7.	28.
1.0	1.5	2.	10.	12.	49.	1.5	2.	7.	9.	33.
0.8	2. +	3. +	15.	21.		2.	3.	10.	11.	42.
0.6	4.	6.	25. +	32.		3. +	5.	14.	14.	56.
0.5	6.	8.	33.	40.		4.	6.5	15.5	14. +	60.
0.4	9.	11.5				6.	9.	17.	13. *	58. *
0.3	15.	18.				8.	11.	11. *		

5° of attraction = 1 grain.

By referring to the attractive forces, it is readily perceptible that the law of the inverse square of the distance is manifest through all the approximations, except a few of the last. It may be supposed, that the inductive influence which thus begins to vary, may, even before contact, so far vanish, as to be in an inverse *simple* ratio of the distance; and this was found by experiment to obtain. It is interesting to examine the law of distribution, which obtains in different points of the longitudinal axis of a magnet of regular figure between the centre and poles. Two bars of steel were regularly hardened throughout and magnetized; the poles of each separate bar being equal, and the magnetic centre in each equidistant from the poles. The line which divided the magnetic curves (obtained by sifting iron filings on a sheet of paper tightly strained above,) was observed, and equal portions on each side thereof marked off. These bars, *m* and M, were placed horizontally beneath the suspended iron A,—the distance A *m* = 2, and A M = 3.

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* In these cases the repulsive force was superseded by attraction.

TABLE XII.

m Distance of point from centre of bar in 0.5 in.	Force.	M Distance of point from centre of bar in 0.5 in.	Force.
1.	2.	1	0.5
2.	4.	2	2.0
3.	9.	3	4. +
4.	16.	4	8.
5.	25.		
6.	36.		

Hence it appears that the law of the distribution varies *directly as the square of the distance from the magnetic centre.* But although this obtains in a bar REGULARLY hardened and magnetized, it is not developed with so much certainty in bars of which the temperament and texture are *irregular*. In order to avoid the interference of angular forces, which *m* and M might have impressed on A, a small piece of soft iron was placed successively on each point in the bar under experiment, thus preserving A from such angular action; the distance of A from the top of the iron being 0.2 inch.

TABLE XIII.

Distance from magnetic centre.	Force.
1.	0.5
2.	2.0
3.	4.5
4.	8.0
5.	12.0+
6.	18.0
7.	25.0
8.	32.0

Two masses of iron I *i*, were placed horizontally between, and under the influence of the dissimilar poles of two magnets *m* M, so as to induce in them equal intensities, as measured by the attractive force on the suspended iron A. On bringing the opposed extremities of these two induced magnets *i* I, in contact, their free action was reduced to zero; by withdrawing them from each other, the free action of either could be estimated: 1*st*, the distance *m* M, and the induced force being constant, and the distance I *i* variable; 2*d*, I *i* constant, and M *m* and the induced force variable; and, 3*d*, when both the distances and the induced force were varied, *i. e.* when I *i* and *m* M were simultaneously varied. The experiment being thus arranged, the forces indicated by *i* were found to vary with the distance I *i*, when the induced forces were constant; and with the induced forces when the distance I *i* was constant; and with these quantities conjointly when they were both made variable. "Now by whatever artificial method we suppose a bar of steel to be made a magnet, whether by the single or double touch, it would not be difficult to show that the first states of the magnetic disturbance, as measured from the magnetic centre, would be in arithmetical progression. The forces impressed on each side of zero may at first be conceived to go on in the arithmetical progression 1, 2, 3, or 1′, 2′, 3′, &c. If we conceive all these forces united in the centre, their free action would be zero. Let us now suppose these opposite forces to be withdrawn through the distances 11′, 22′, 33′, &c. successively; then, by the preceding experiments, the forces set free in the points 1, 2, 3, &c. 1′, 2′, 3,′ &c. would vary directly as the square of the distances from the magnetic centre, since they vary directly with the magnetic intensities and directly with the distances.

Art. III.—*Notice of the performance of Steam-Engines in Cornwall for January, February, and March* 1830. By W. J. Henwood, F. G. S., Member of the Royal Geological Society of Cornwall. Communicated by the Author.

Reciprocating Engines drawing Water.

Mines.	Diameter of cylinder in inch.	Length of stroke in cylinder in feet.	Length of stroke in the pump in feet.	Load in lbs. per sq. in. of area of piston.	No. of strokes per minute.	Millions of lbs. weight lifted 1 foot high by the consumption of 1 bush. of coal.
Stray Park, -	64	7,75	5,25	7,	4,9	24,1
Huel Vor, -	63*	7,25	5,75	17,5	5,5	27,2
	53	9,	7,5	19,5	5,	43,2
	48	7,	5,	8,	5,	29,6
	80	10,	7,5	13,5	6,1	61,7
	45	6,75	5,5	13,7	6,6	48,7
Poladras Downs,	70	10,	7,5	9,4	7,	54,1
Huel Reeth,	36	7,5	7,5	15,3	3,7	24,1
Balnoon, -	30	8,	7,	9,	2,8	20,6
Huel Towan, -	80	10,	8,	11,2	6,1	75,5
	80	10,	8,	6,7	4,3	64,9
United Hills, -	58	8,25	6,5	7,6	4,3	38,5
Crinis, -	56†	6,75	6,75	9,1	5,8	44,3
Huel Unity, -	52	6,666	5,75	9,1	6,4	33,7
	60	7,25	5,75	13,8	5,3	32,9
Poldice, -	90	10,	7,	10,5	5,3	46,6
	60	9,5	6,25	12,8	5,9	47,3
Huel Damsel, -	42‡	7,5	5,75	21,	4,6	32,9
	50	9,	7,	9,6	3,2	32,4
Ting Tang, -	63	8,	6,	14,2	4,4	45,3
	66	9,	7,5	11,6	3,3	44,9
Cardrew Downs,	66	8,75	7,	10,4	5,9	58,4
Huel Harmony,	70	9,25	7,	8,8	5,2	35,1
Huel Montague,	50	9,	7,	11,	2,5	26,
Dolcoath, -	76	9,	7,5	11,8	4,8	41,5
Great Work, -	60	9,	7,	10,2	5,9	39,9
Huel Penrose,	36	8,5	6,5	13,1	7,4	29,1
Huel Caroline,	30	7,	6,	29,3	8,4	32,1
	53,5	8,333	7,	7,6	5,5	24,

Mines.	Diameter of cylinder in inch.	Length of stroke in cylinder in feet.	Length of stroke in the pump in feet.	Load in lbs. per sq. in. of area of piston.	No. of strokes per minute.	Millions of lbs. weight lifted 1 foot high by the consumption of 1 bush. of coal.
St. Ives Consols,	36	7,	7,	16,	6,1	27,9
Lelant Consols,	15	7,5	4,5	17,2	2,6	10,5
Binner Downs,	70	10,	7,5	10,9	8,	61,
	64	9,333	7,75	7,7	9,6	51,3
	42	9,	7,5	13,8	7,	40,4
Consolidated Mines,	90	10,	7,5	8,8	5,5	58,9
	70	10,	7,5	10,4	6,4	62,3
	65	9,	7,5	15,9	3,4	59,6
	90	10,	7,5	8,7	7,4	59,
	90	10,	7,5	10,3	3,4	38,7
	65	9,	7,5	12,4	4,9	64,7
United Mines,	90	9,	8,	7,9	4,6	43,9
	30	9,	7,5	12,9	8,3	42,4
Huel Beauchamp,	36	7,75	6,	13,5	4,7	36,
Huel Rose,	60	9,	7,	14,	6,6	60,1
Pembroke,	80	9,75	7,25	11,3	3,4	48,8
	50	9,	7,	10,4	7,1	49,4
East Crinnis,	60	5,5	5,5	8,5	3,8	20,7
	70	10,	7,	9,4	3,5	33,8
East Huel Unity,	45	8,75	6,75	13,1	3,6	38,4
Huel Hope,	60	9,	8,	11,7	5,6	53,5
Huel Tolgus,	70	10,	7,5	8,4	5,5	57,4
Tresavean,	60	9,	7,	7,7	4,5	22,4
Huel Falmouth,	58	8,75	6,5	4,1	4,1	24,2
Huel Sperris,	70	10,333	7,75	6,7	6,1	39,5
Huel Prosper,	53	7,	7,	4,2	5,	26,6
Huel Leisure,	36	9,25	6,75	14,3	7,9	35,9
	70	9,833	7,75	5,1	3,9	38,
Marazion Mines,	60	9,	8,	5,2	8,2	47,8

Average duty 41,58 millions of lbs. weight lifted one foot high by the consumption of one bushel (84 lbs.) of coal.

Watt's rotatory double engines employed to move machinery for bruising tin ores.

			Length of crank.			
Huel Vor,	24.	6.	6.	12.	16.1	17.7
	27.	5.	5.	12.	17.6	19.3
	16.5	5.	5.	8.5	26.6	12.6

Average duty of rotatory engines, 16.5 millions.

* Watt's double engines.

† Trevithick's high pressure combined with Watt's single —for March, Watt's single only.

† For one month only.

Erratum.—In the Report for July, August, and September 1829, Huel Damsel Engine, 50 inches cylinder, *for* duty 136.6 millions, *read* 36.6 millions.

ART. IV.—*Notice of experiments with Laurel Oil.* By Dr HANCOCK. Communicated by the Author.

I BELIEVE it is understood to be a fundamental principle in hydrostatics, that when two *immiscible* fluids are added together, the heavier of the two has a tendency to displace the other, or to occupy the bottom of the vessel. It appears indeed, a self-evident truth, and scarcely necessary to be stated as an axiom, founded as it is upon the universal principle of gravitation. It may therefore appear paradoxical to assert the contrary of any two bodies,—and I have hesitated whether I could safely state the following fact, being apprehensive of some fallacy in my own senses or mode of considering the subject, and it is chiefly with a view of acquiring information, that I am now induced to offer it to the attention of the scientific chemist.

The fact I wish to notice is this: When we add together a portion of sulphuric ether and proof spirit in equal parts, and of laurel oil, the latter, although a fluid of greater specific gravity, invariably occupies the upper part, *or floats upon the surface of the compound of ether and proof spirit.* The result is identical whether we pour in the oil or the compound first, and in whatever proportions they may be added,—and whether we shake the vial, or put them together in the most gentle manner.[*] How is this to be accounted for? Is not the buoyancy of fluids an invariable criterion of their specific gravity?

Intending to try the effect of *washed ether* with the native oil, I added to water some ether from a druggist's shop in George Town.—I found I could not wash this ether, for it mixed or combined with water in every proportion, I hence concluded it to be impure, or a compound of ether and diluted alcohol.[†] I then tried by the same test, a portion from another druggist's, and I found I could wash this ether; and that it only took up about a tenth of its bulk of water,—of course it proved to be genuine.

The next step was to mix the *genuine ether* with strong spirit. I added equal parts of these together; they formed a clear transparent solution or mixture. The spirit was double distilled rum, *proof* 23,[‡] by the glass bubble,—the only hydrometer I could obtain, and indeed the only one I know of conveniently adapted for proving the specific gravity of small portions of fluids.

I found this mixture of ether and proof spirit to sink the bubble 17, then adding this to the native oil of proof 21, the laurel oil was found to float upon the surface of the ethereal alcoholic mixture, although it was 4 degrees or 4 bubbles heavier, as indicated by the glass bead hydrometer; if this may be considered any test of the specific gravity of fluids.

The steps by which we arrive at a certain result, may often reveal others not less interesting than the object directly sought for. In the course of this examination I have found, that the greater part of our ether is adulterated either here, or before it is sent out from Europe. Of four different samples I have recently tried, only one has proved to be genuine; two of these samples appeared to contain less than a fourth part of real ether.

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[*] The phenomena cannot therefore be the result of tenacity or cohesion.

[†] This it may be useful to notice as a test of genuine ether.

[‡] I afterward employed strong high wines and ether in equal parts, and with the same result but slower. I also find it identical in the result, whether we employ the native oil in its usual specific gravity, *i. e.* proof 18, or that which by long standing and exposure has acquired the gravity of 21 or more,—in the latter case, exceeding the ethereal alcoholic mixture on which it swims, by 5 or 6 grades of the hydrometer.

With genuine ether the laurel oil dissolves or mixes in every proportion, but not so if the ether is in the smallest degree adulterated. Subsequently I found a similar result with high wines and laurel oil, but slow, and less sensible, the affinities of the oil being much weaker for alcohol than for ether. Should there be a portion of water or spirit mixed with the ether, it becomes sensible in the turbid mixture ensuing on the addition of the laurel oil.

The composition which, after many trials, I find to produce the effect with the most striking celerity, is that with equal parts of ether and common spirit, *proof* 28. It will be good to add the ether in full equal proportion, or rather exceeding that of the spirit, to render the effect more certain and conclusive.

The compound with brandy, proof about 26, does very well, but not equal to that of proof 30. *With spirit proof 32 the ether does not mix at all*, remaining separate in the same volume as added.

In these results, there can be no doubt, I think, that the seemingly strange phenomena proceed from the strong affinity and combination of the laurel oil with ether, by which it is attracted and separated from the water and alcohol. This was afterwards proved by adding together pure ether and pure laurel oil, when I found them intimately combine as already observed.

I have also observed that the laurel oil evaporated to the consistence of Marana balsam will also float on the same compound, and the Marana, or Balsam Copaivi even to rise slowly and float on it. On adding the Marana, and preparatory to its rising in the ethereal compound, we see an active chemical process take place.

In an experiment with iodine and laurel oil, by throwing the former on the latter, a violent action or crepitus was instantly produced, bordering on combustion, or rather like sparks of fire or a fine coal thrown into water. At the instant of contact, much heat was evolved, and a bituminous odour, (as of hydrogen gas,) but no light; the oil received a bright red colour, but extremely fugacious, and in a few minutes the colour entirely vanished. It is probable that hydrogen gas was disengaged on the decomposition of the fluid by the iodine.

In another trial, when iodine was added in a larger proportion, the result was a black substance similar to petroleum or Barbadoes tar, in colour, taste, and smell.

Many naturalists have suspected amber and the bituminous substances to be of vegetable origin. The striking analogy above noticed, brought this more forcibly to mind, and impressed me with the idea that such might be the case,—that bitumens might also be the result of vegetable exudations, variously modified by the agency of soda and divers minerals? Or is it more rational and natural to conclude, that, on the contrary, these substances exist in the original structure of the earth, and that they are absorbed by the roots of vegetables, exuding under various modifications, as varied as the vegetables which give them out?* If such were the case, we might expect to find some traces of the bitumens in the soil where such resinoids or terebinthous vegetables abound, unless they have the property of elaborating these various resinoid matters from other elements.

It would be interesting, and perhaps throw some light on the subject, if the soil where these kind of trees grow most abundantly, were carefully analyzed with this view and compared with that of other parts.

The compound of iodine and laurel oil coloured spirits of wine of a light amber tint, but does not dissolve in it nor in water.

ART. V.—*Account of a curious phenomenon of revolving motions, produced by the combination of Alcohol with Laurel Oil.* By Dr HANCOCK. Communicated by the Author.

To exhibit a singular spectacle which seems to bear some analogy with the motions of the planetary orbs, take a vial of the laurel oil and drop into it, at different intervals, some rectified spirits of wine, when the most interesting results will be observed to ensue; a circulation presently commencing, of globules of alcohol up and down through the oil, which will last for many hours, or for days, (how long is unknown.) A revolving or circulating motion also appears in the oil, carrying the alcoholic globules through a series of mutual attractions and repulsions,—the round bodies moving freely through the fluid, turning short in a small eccentric curve at each extremity of their course, passing each other rapidily without touching; but after a time, they seem to acquire a density approximating to that of the lower stratum, which appears to be an aqueous portion, separated by the ethereal oil from the alcohol; and this assimilation taking place, the globules, after performing many revolutions, will fall flat upon the surface, and unite with the lower or watery stratum.

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* The Courida tree, (*Avicennia nitida*) is found thus to take up sea salt and deposit it on the leaves;—it is only on salt land, however, where this occurs, as the same tree placed at a distance from the sea gives no salt; and this is the only vegetable on which I have found salt thus deposited. In a severe dry season, the salt is seen like hoar frost on the leaves.

The orbits of those small globules being confined by the glass are very eccentric. In the course of the experiment, I observed particles of the fluid to separate in larger globular portions; these commenced a similar revolution, and smaller ones quitted their course and revolved about the larger, whilst the latter still pursued their course after the manner of primary planets and their secondaries. This, however, can only be well understood by seeing the experiment, which is easily performed, and well worth the trouble; as it appears to me, that, if attentively studied, it might furnish important deductions, and serve, we know not how far, towards an illustration of the celestial motions.

In the present case, the revolving motion of these globules appeared to be, not as we are accustomed to regard the planetary motions, as the effect of a direct attractive and repulsive power, in combination with a projectile force, but as revolving in a circulating medium, attended by an emanation from the globules themselves.

This experiment was performed with a small vial. Perhaps a larger one would render the result more perspicuous.

ART. VI.—*Description of a simple, cheap, and accurate Rain-Gage, calculated to show the depth of rain falling around it to the ten-thousandth part of an inch.* By MATTHEW ADAM, A. M., Rector of the Academy of Inverness, and Associate of the Society of Arts for Scotland. Communicated by the Author.

THIS instrument is composed of four parts, represented in Plate I. Fig. 1. viz. 1st, A painted square-mouthed tin-plate filler, A B *l* C D *m*, for collecting the rain water, having the length of each side, A B, A C, &c. of its mouth equal to ten inches, and consequently its superficial area equal to 100 square inches. About half-an-inch of its mouth is turned perpendicularly up, to prevent any part of the rain which has entered it from being blown out again by wind ; and its throat D is nearly closed by a piece of tin-plate pierced with ten or twelve holes, each about one-eighth of an inch in diameter, to permit the descent of the rain water, and to retard its rising again through the neck of the filler by evaporation.

2d, A large bottle E F G, which admits into its mouth E, a part of D *m*, the neck of the filler, and holds nearly one-half of an imperial gallon, so that it may contain all the rain which at any time may enter it, through the filler, in the course of twelve hours.

3d, A cylindrical glass tube or measure K L, whose inside diameter may be ½, ⅔, or ¾, of an inch. Its lower extremity L, is closed ; its upper extremity K*k*, is enlarged, or funnel-shaped, that the rain may be all easily poured into it from the bottle ; and on one side, it is accurately graduated from L to K, into portions having the capacity of cubic inches and tenths of a cubic inch.

4th, An inch plank, or post D I, of convenient length, firmly fixed vertically in a sheltered situation selected for it, with three appendages, viz. *1st*, A horizontal shelf H G, perpendicular to D I, and about 3½ feet above ground, for the purpose of supporting the bottle. *2d*, A bent iron hoop *c d e*, screwed or nailed to the post at *c* and *e*, for the purpose of holding the bottle firmly in its place, when exposed to storm. And *3d*, Two strong iron wires *f g*, *h i*, driven or screwed into the post at *g* and *i*, and bent into circular apertures at *f* and *h*, so as conveniently to hold the graduated glass measure K L, that it may be ready, when required, to ascertain the number of cubic inches, and tenths, and hundredths, of a cubic inch of the water which has entered the bottle, and consequently also the depth of the rain then fallen in the adjacent country, in hundredth, thousandth, and ten-thousandth parts of an inch.

Explanation.—The superficial area of the mouth of the filler being 100 square inches, it is obvious that 100 cubic inches of rain water must enter through it into the bottle, when one inch deep of rain falls in the adjacent country, and that every cubic inch of this water, being the hundredth part of the whole, must indicate the hundredth part of an inch deep of rain. Consequently every tenth and hundredth part of a cubic inch of the same water, measured in the graduated glass tube K L, must likewise indicate the thousandth and ten-thousandth part of an inch deep of rain.

If the inside diameter of the cylindrical glass measure K L be only half-an-inch, the circular area of a section of it, viz. .19635, or a little less than $\frac{1}{5}$ of a square inch, will be contained 509, and nearly $\frac{1}{3}$ times in 100 square inches, the area of the square mouth of the filler; and, as the depth of measures of equal capacity are reciprocally as the areas of their bases, or corresponding sections, it is clear, that, to measure 100 cubic inches of rain water, which may be contained in one inch deep of the square mouth of the filler, there would be required a depth or length of 509 inches of the cylindrical glass measure, whose diameter is only half an inch. Consequently the hundredth part of this length, or five inches and nearly $\frac{1}{10}$ of an inch of this measure, will be required to contain one cubic inch, or to measure the hundredth part of an inch deep of rain. Half-an-inch of it will be required to contain $\frac{1}{10}$ of a cubic inch, or to measure the thousandth part of an inch deep of rain; and consequently the tenth part of half-an-inch or a length equal to $\frac{1}{20}$ of an inch of this measure, will be required to contain the hundredth part of a cubic inch or to measure the ten-thousandth part of an inch deep of rain. And it is obvious that a depth, or length, much smaller than the twentieth of an inch can easily be distinguished by the eye without the aid of a magnifier.

If a similar measure of $\frac{5}{8}$ or $\frac{3}{4}$ of an inch in diameter be graduated in the same manner, the ultimate divisions of such measure, even when carried only to tenths of a cubic inch, are therefore sufficient to enable a careful observer to determine the depth of rain fallen around the gage, to the ten-thousandth part of an inch.

In regard to expence.—The tin-plate filler, the large bottle, and the wooden post with its appendages, may all, it is believed, be obtained for 3s. 6d. or little more than one shilling each; and the glass measure, properly graduated, may be purchased at Knight's Chemical Instrument Warehouse, in London, for 2s. or 2s. 6d. The whole may therefore be procured for about 6s.—a price extremely moderate compared with that of the rain-gages usually sold at four or five guineas each, and which show the depth of rain falling around them only to the hundredth part of an inch.

If only a graduated measure, a tin-plate filler, and a common bottle, sunk four or five inches in the ground were used, the whole expence would probably not exceed 3s. 6d. or 4s. But during great rains a small bottle would not contain all the rain entering it through the filler in the course of only five or six hours; a circumstance which would render the use of a small bottle very inconvenient, particularly during the night.

Mr Adam's rain-gage, of the kind now described, has been used since the 18th of September last, and appears to him to answer its purpose well. For though the mean inside diameter of the graduated measure is upwards of $\frac{3}{4}$ or nearly .9 of an inch, and diminishes a little from top to bottom, so that the distance between the divisions, marking $\frac{1}{10}$ of a cubic inch, varies from $\frac{5}{40}$ to $\frac{8}{40}$ of an inch; yet he can easily observe by it the depth of rain falling around the gage to the $\frac{1}{10000}$ part of an inch.

The depth of rain is therefore entered in the register in inches and four decimals of an inch; the first two decimals being obtained from the cubic inches, and the last two from the tenths and decimals of the tenth of a cubic inch, marked on the graduated measure. Thus a quantity of rain measuring seventeen cubic inches, eight-tenths, and eight-hundredths of a cubic inch in the graduated measure, is marked in the register .1788 of an inch deep of rain, and a quantity measuring only two-hundredths of a cubic inch in the graduated measure, is marked in the register .0002, or two ten-thousandth parts of an inch deep of rain. This last, it is true, is an extremely minute quantity of rain. But there are many such minute entries in the register, varying from one to fifteen tenthousanth parts of an inch. And it is remarkable, that most of them are noted as miniature showers, or depositions of dew, from the air inclosed in the bottle; because these miniature showers or depositions of dew were then observed to vary in quantity with the variations of sky and temperature, and to take place frequently at periods when the sky was clear, and always when so entered, at times when no rain fell from the external air. These entries, thus noticed, may therefore, it is apprehended, be considered as measurements of the varying quantities of dew deposited by the air contained in the bottle, under the varying temperatures and states of the weather then noted in the register.

Explanatory Notes.

1*st*, 10 inches × 10 = 100 square inches, = the area of the square mouth of the filler of Mr Adam's rain-gage.

$\frac{1}{2} \times 3.1416 \times \frac{1}{2} \times \frac{1}{4} = .7854 \times \frac{1}{4} = .19635$ of a square inch, = the area of a circular section of a tube whose diameter $= \frac{1}{2}$ inch.

Now 100×1 inch deep $= .19635 \times l$ inches deep. Therefore $100 \div .19635 = 509.2946$, or 509.3 lineal inches nearly $= l$ = the length of a glass tube, (whose diameter $= \frac{1}{2}$ inch,) required to contain 100 cubic inches, or to measure 1 inch deep of rain.

$\frac{l}{100} = 5.09$ inches $= 5\frac{1}{11}$ inches nearly = length of tube containing 1 cubic inch, and measuring one-hundredth part of an inch deep of rain, $= m$.

$\frac{m}{10} = \frac{5}{10} = \frac{1}{2}$ inch = length containing $\frac{1}{10}$ of a cubic inch, and measuring $\frac{1}{1000}$ part of an inch deep of rain, $= n$.

and $\frac{n}{10} = \frac{1}{2} \div 10 = \frac{1}{20}$ of an inch of the said tube, which contains $\frac{1}{100}$ part of a cubic inch of rain water, and measures $\frac{1}{10000}$ part of an inch deep of rain.

2*d*, Mean diameter of the circular mouth of the Academy Rain-Gage, made by Mr Adie of Edinburgh, = 6.8 inches.

Mean diameter of the cylindrical brass tube below and supporting it, made by Mr Adie, = 2.075 inches.

Mean diameter of the glass tube, parallel to do. (and to which the graduated brass scale is applied,) made by Mr Adie, = .546 inches.

Therefore $6.8 \times 3.1416 \times \frac{1}{2} \times 3.4 = 36.316896$ square inches, = the area of the mouth of the rain-gage $= a$,

$2.075 \times 3.1416 \times \frac{1}{2} \times 1.0375 = 3.3817378$ square inches = the area of a circular section of the brass tube $= b$,

$.564 \times 3.1416 \times \frac{1}{2} \times .282 = .2498326$ square inches, = the area of a circular section of the glass tube $= c$,

Now $a \times 1$ inch deep $= (b + c) \times l$ inches deep.

Therefore $a \div (b + c) = 36.316896 \div 3.6315704 = l = 10$ inches, = the length of the scale measuring one inch deep of rain, $\frac{l}{10} = 1$ inch, = the length of the scale measuring $\frac{1}{10}$ inch deep of rain, and $\frac{l}{100} = \frac{1}{10}$ inch, = the length of the scale measuring $\frac{1}{100}$ part of an inch deep of rain. The mean inside diameter of the graduated cylindrical glass measure, at present in use for Mr Adam's rain-gage, being nearly $\frac{9}{10}$ of an inch, the area of a circular section of it must be equal to $.9 \times 3.1416 \times \frac{1}{2} \times .45 = .636174$ of a square inch nearly $= d$. Now $(b + c) \times 1$ inch deep, $= d \times l$ inches deep. Wherefore $\frac{b + c}{d} = \frac{3.6315704}{.636174} = 5.7$ lineal inches nearly $= l =$ the length of the said graduated glass measure required to fill the tubes b, and c, one inch deep, = the length of four cubic inches in the said graduated glass nearly. Consequently four cubic inches of water ought to fill the tubes b, and c, of Mr Adie's rain-gage, to the depth of one inch, which upon his scale indicates a depth of rain equal to $\frac{1}{10}$ of an inch; and $\frac{4}{10}$ of a cubic inch ought therefore to fill the tubes b, and c, to the depth of $\frac{1}{10}$ of an inch, which on the same scale marks the hundredth part of an inch deep of rain.

P. S. At the monthly meeting of the Northern Institution, held at Inverness, on Friday 29th January 1830, the foregoing description and explanatory notes were read and explained by Mr Adam. The academy rain-gage, made by

Alexander Adie, Esq. optician, Edinburgh, and that made by Mr Adam, were both exhibited, and the method of using them explained. But the construction and use of the academy rain-gage was *first* explained, and by several experiments. shown to agree very exactly with the description of it contained in the second of the above explanatory notes, in order to enable the meeting *afterwards* more easily to understand and judge of the advantages alleged to be gained by Mr Adam's altered construction of the instrument.

By means of a tinplate measure containing exactly a cubic inch of water, the accuracy of the graduation of Mr Adam's cylindrical glass measure, (made by Mr Knight of London,) into portions containing cubic inches, and decimals of a cubic inch, were distinctly shown, and an easy practical method of correctly graduating a cylindrical glass measure of any diameter was likewise described, and illustrated by experiments.

ART. VII.—*Observations on the National Encouragement of Science, and on its Encouragement by learned Societies, with observations on the general state of learned Societies in England.* By CHARLES BABBAGE, Esq. M. A. F. R. S. L. and E., Lucasian Professor of Mathematics, Cambridge.*

THE little encouragement which at all previous periods has been afforded by the English Government to the authors of useful discoveries, or of new and valuable inventions, is justified on the following grounds:

1. The public, who consume the new commodity or profit by the new invention, are much better judges of its merit than the government can be.

2. The reward which arises from the sale of the commodity is usually much larger than that which government would be justified in bestowing; and it is exactly proportioned to the consumption, that is, to the want which the public feel for the new article.

It must be admitted that, as general principles, these are correct: there are, however, exceptions which flow necessarily from the very reasoning from which they were deduced.

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* From his work "*on the Decline of Science.*"

Without entering minutely into these exceptions, it will be sufficient to show that all abstract truth is entirely excluded from reward under this system. It is only the application of principles to common life which can be thus rewarded. A few instances may perhaps render this position more evident. The principle of the hydrostatic paradox was known as a speculative truth in the time of Stevinus;* and its application to raising heavy weights has long been stated in elementary treatises on natural philosophy, as well as constantly exhibited in lectures. Yet, it may fairly be regarded as a mere abstract principle, until the late Mr Bramah, by substituting a pump instead of the smaller column, converted it into a most valuable and powerful engine.—The principle of the convertibility of the centres of oscillation and suspension in the pendulum, discovered by Huygens more than a century and a half ago, remained, until within these few years, a sterile, though most elegant proposition; when, after being hinted at by Prony, and distinctly pointed out by Bonenberger, it was employed by Captain Kater as the foundation of a most convenient practical method of determining the length of the pendulum.—The interval which separated the discovery, by Dr Black, of latent heat, from the beautiful and successful application of it to the steam engine, was comparatively short; but it required the efforts of two minds; and both were of the highest order.—The influence of electricity in producing decompositions, although of inestimable value as an instrument of discovery in chemical inquiries, can hardly be said to have been applied to the practical purposes of life, until the same powerful genius which detected the principle, applied it, by a singular felicity of reasoning, to arrest the corrosion of the copper-sheathing of vessels. That admirably connected chain of reasoning, the truth of which is confirmed by its very failure as a remedy, will probably at some future day supply, by its successful application, a new proof of the position we are endeavouring to establish.

Other instances might, if necessary, be adduced, to show that long intervals frequently elapse between the discovery of new principles in science and their practical application: nor ought this at all to surprise us. Those intellectual qualifications, which give birth to new principles or to new methods, are of quite a different order from those which are necessary for their practical application.

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* About the year 1600.

At the time of the discovery of the beautiful theorem of Huygens, it required in its author not merely a complete knowledge of the mathematical science of his age, but a genius to enlarge his boundaries by new creations of his own. Such talents are not always united with a quick perception of the details, and of the practical applications of the principles they have developed, nor is it for the interest of mankind that minds of this high order should lavish their powers on subjects unsuited to their grasp.

In mathematical science, more than in all others, it happens that truths which are at one period the most abstract, and apparently the most remote from all useful application, become in the next age the bases of profound physical inquiries, and in the succeeding one, perhaps, by proper simplification and reduction to tables, furnish their ready and daily aid to the artist and the sailor.

It may also happen that at the time of the discovery of such principles, the mechanical arts may be too imperfect to render their application likely to be attended with success. Such was the case with the principle of the hydrostatic paradox; and it was not, I believe, until the expiration of Mr Bramah's patent, that the press which bears his name received that mechanical perfection in its execution, which has deservedly brought it into such general use.

On the other hand, for one person who is blessed with the power of invention, many will always be found who have the capacity of applying principles; and much of the merit ascribed to these applications will always depend on the care and labour bestowed in the practical detail.

If, therefore, it is important to the country that abstract principles should be applied to practical use, it is clear that it is also important that encouragement should be held out to the few who are capable of adding to the number of those truths on which such applications are founded. Unless there exist peculiar institutions for the support of such inquirers, or unless the Government directly interfere, the contriver of a thaumatrope may derive profit from his ingenuity, whilst he who unravels the laws of light and vision, on which multitudes of phenomena depend, shall descend unrewarded to the tomb.

Perhaps it may be urged, that sufficient encouragement is already afforded to abstract science in our different universities, by the professorships established at them. It is not, however, in the power of such institutions to create; they may foster and aid the developement of genius: and, when rightly applied, such stations ought to be its fair and honourable rewards. In many instances their emolument is small; and when otherwise, the lectures which are required from the professor are not perhaps in all cases the best mode of employing the energies of those who are capable of inventing.

I cannot resist the opportunity of supporting these opinions by the authority of one of the greatest philosophers of a past age, and of expressing my acknowledgments to the author of a most interesting piece of scientific biography. In the correspondence which terminated in the return of Galileo to a professorship in his native country, he remarks, "But, because my private lectures and domestic pupils are a great hinderance and interruption of my studies, I wish to live entirely exempt from the former, and in great measure from the latter."—*Life of Galileo*, p. 18. And, in another letter to Kepler, he speaks with gratitude of Cosmo, the Grand Duke of Tuscany, who "has now invited me to attach myself to him with the annual salary of 1000 florins, and with the title of Philosopher and principal Mathematician to his Highness, without the duties of any office to perform, but with most complete leisure; so that I can complete my treatise on Mechanics, &c."—P. 31. *

Surely, if knowledge is valuable, it can never be good policy in a country far wealthier than Tuscany, to allow a genius like Mr Dalton's, to be employed in the drudgery of elementary instruction. Where would have been the military renown of England, if with an equally improvident waste of mental power, its institutions had forced the Duke of Wellington to employ his life in drilling recruits, instead of planning campaigns?

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* *Life of Galileo*, published by the Society for the Diffusion of Useful Knowledge.

If we look at the fact, we shall find that the great inventions of the age are not, with us at least, always produced in universities. The doctrines of "definite proportions," and of the "chemical agency of electricity,"—principles of a high order, which have immortalized the names of their discoverers,—were not produced by the meditations of the cloister: nor is it in the least a reproach to those valuable institutions to mention truths like these. Fortunate circumstances must concur, even to the greatest, to render them eminently successful. It is not permitted to all to be born, like Archimedes, when a science was to be created; nor like Newton, to find the system of the world "without form and void;" and, by disclosing *gravitation*, to shed throughout that system the same irresistible radiance as that with which the Almighty Creator had illumined its material substance. It can happen to but few philosophers, and but at distant intervals, to snatch a science, like Dalton, from the chaos of indefinite combination, and binding it in the chains of number, to exalt it to rank amongst the exact. Triumphs like these are necessarily "few and far between;" nor can it be expected that that portion of encouragement, which a country may think fit to bestow on science, should be adapted to meet such instances. Too extraordinary to be frequent, they must be left, if they are to be encouraged at all, to some direct interference of the government.

The dangers to be apprehended from such a specific interference, would arise from one, or several, of the following circumstances:—That class of society, from whom the government is selected, might not possess sufficient knowledge either to judge themselves, or know upon whose judgment to rely. Or the number of persons devoting themselves to science, might not be sufficiently large to have due weight in the expression of public opinion. Or, supposing this class to be large, it might not enjoy, in the estimation of the world, a sufficiently high character for independence. Should these causes concur in any country, it might become highly injurious to commit the encouragement of science to any department of the government. This reasoning does not appear to have escaped the penetration of those who advised the abolition of the late Board of Longitude.

The question whether it is good policy in the government of a country to encourage science, is one of which those who cultivate it are not perhaps the most unbiassed judges. In England, those who have hitherto pursued science, have in general no very reasonable grounds of complaint; they knew, or should have known, that there was no demand for it, that it led to little honour, and to less profit.

That blame has been attributed to the government for not fostering the science of the country is certain; and, as far as regards past administrations, is, to a great extent, just; with respect to the present ministers, whose strength essentially depends on public opinion, it is not necessary that they should precede, and they cannot remain long insensible to any expression of the general feeling. But supposing science were thought of some importance by any administration, it would be difficult in the present state of things to do much in its favour; because, on the one hand, the higher classes in general have not a profound knowledge of science, and, on the other, those persons whom they have usually consulted, seem not to have given such advice as to deserve the confidence of government. It seems to be forgotten, that the money allotted by government to purposes of science ought to be expended with the same regard to prudence and economy as in the disposal of money in the affairs of private life.

To those who measure the question of the national encouragement of science by its value in pounds, shillings, and pence, I will here state a fact, which, although pretty generally known, still, I think, deserves attention. A short time since it was discovered by government that the terms on which annuities had been granted by them were erroneous, and new tables were introduced by act of Parliament. It was stated at the time that the erroneous tables had caused a loss to the country of between two and three millions sterling. The fact of the sale of those annuities being a losing concern was long known to many; and the government appear to have been the last to have been informed on the subject. Half the interest of half that loss, judiciously applied to the encouragement of mathematical science, would, in a few years, have rendered utterly impossible such expensive errors.

To those who bow to the authority of great names, one remark may have its weight. The *Mecanique Celeste*,* and the *Théorie Analytique des Probabilités*, were both dedicated, by Laplace, to Napoleon. During the reign of that extraordinary man, the triumphs of France were as eminent in science as they were splendid in arms. May the institutions which trained and rewarded her philosophers be permanent as the benefits they have conferred upon mankind!

In other countries it has been found, and is admitted, that a knowledge of science is a recommendation to public appointments, and that a man does not make a worse ambassador because he has directed an observatory, or has added by his discoveries to the extent of our knowledge of animated nature. Instances even are not wanting of ministers who have begun their career in the inquiries of pure analysis. As such examples are perhaps more frequent than is generally imagined, it may be useful to mention a few of those men of science who have formerly held, or who now hold, high official stations in the governments of their respective countries.

Country.	Name.	Department of Science.	Public Office.
France .	Marquis Laplace† . .	Mathematics . .	President of the Conservative Senate.
France . .	M. Carnot	Mathematics . .	Minister of War
France . .	Count Chaptal ‡ . .	Chemistry . . .	Minister of the Interior.
France . .	Baron Cuvier § . . .	Comparative Anatomy, Natural History	Minister of Public Instruction.
Prussia . .	Baron Humboldt . .	Oriental Languages	Ambassador to England.

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* The first volume of the first translation of this celebrated work into our own language, has just arrived in England from ——— America.

† Author of the *Mecanique Celeste.*

‡ Author of *Traité de Chimie Appliqué aux Arts.*

§ Author of *Leçons d'Anatomie Comparée—Récherches sur les Ossemens Fossiles, &c. &c.*

Country.	Name.	Department of Science.	Public Office.
Prussia . .	Baron Alexander Humboldt	The celebrated Traveller	Chamberlain to the King of Prussia.
Modena . .	Marquis Rangoni[*]	Mathematics . .	Minister of Finance and of Public Instruction, President of the Italian Academy of Forty.
Tuscany .	Count Fossombroni[†]	Mathematics . .	Prime Minister of the Grand Duke of Tuscany.
Saxony . .	M. Lindenau[‡] . . .	Astronomy . . .	Ambassador.

M. Lindenau, the Minister from the King of Saxony to the King of the Netherlands, commenced his career as astronomer at the observatory of the Grand Duke of Gotha, by whom he was sent as his representative at the German Diet. On the death of the late reigning Duke, M. Lindenau was invited to Dresden, and filled the same situation under the King of Saxony; after which he was appointed his minister at the court of the King of the Netherlands. Such occurrences are not to be paralleled in our own country, at least not in modern times. Newton was, it is true, more than a century since, appointed Master of the Mint; but let any person suggest an appointment of a similar kind in the present day, and he will gather from the smiles of those to whom he proposes it that the highest knowledge conduces nothing to success, and that political power is almost the only recommendation.

Of Encouragement from Learned Societies.

There are several circumstances which concur in inducing persons pursuing science, to unite together, to form societies or academies. In former times, when philosophical instruments were more rare, and the art of making experiments was less perfectly known, it was almost necessary. More recently, whilst numerous additions are constantly making to science, it has been found that those who are most capable of extending human knowledge, are frequently least able to encounter the expence of printing their investigations. It is therefore convenient, that some means should be devised for relieving them from this difficulty, and the volumes of the Transactions of academies have accomplished the desired end.

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[*] Author of *Memoria sulle Funzioni Generatrici*, Modena, 1824, and of various other memoirs on mathematical subjects.

[†] Author of several memoirs on mechanics and hydraulics, in the *Transactions of the Academy of Forty*.

[‡] Author of *Tables Barometriques*, Gotha, 1809—*Tabulæ Veneris, novæ et correctæ*, Gothæ, 1810—*Investigatio Nova Orbitæ a Mercurio circa Solem descriptæ*, Gothæ, 1813, and of other works.

There is, however, another purpose to which academies contribute. When they consist of a limited number of persons, eminent for their knowledge, it becomes an object of ambition to be admitted on their list. Thus a stimulus is applied to all those who cultivate science, which urges on their exertions, in order to acquire the wished-for distinction. It is clear that this envied position will be valued in proportion to the difficulty of its attainment, and also to the celebrity of those who enjoy it; and whenever the standard of scientific knowledge which qualifies for its ranks is lowered, the value of the distinction itself will be diminished. If, at any time, a multitude of persons having no sort of knowledge of science are admitted, it must cease to be sought after as an object of ambition by men of science, and the class of persons to whom it will become an object of desire will be less intellectual.

Let us now compare the numbers composing some of the various academies of Europe.—The Royal Society of London, the Institute of France, the Italian Academy of Forty, and the Royal Academy of Berlin, are amongst the most distinguished.

Name. Country.	Population.	Number of Members of its Academy.	Number of Foreign Members.
1. England . .	22,299,000	685	50
2. France . . .	32,050,000	75	8 Mem. 100 Corr.
3. Prussia . . .	12,415,000	38	16
4. Italy	12,000,000	40	8

It appears then, that in France, one person out of 427,000 is a member of the Institute. That in Italy and Prussia, about one out of 300,000 persons is a member of their academies. That in England, every 32,000 inhabitants produces a Fellow of the Royal Society. Looking merely at these proportions, the estimation of a seat in the Academy of Berlin, must be more than nine times as valuable as a similar situation in England; and a member of the Institute of France will be more than thirteen times more rare in his country than a Fellow of the Royal Society is in England.

Favourable as this view is to the dignity of such situations in other countries, their comparative rarity is by no means the most striking difference in the circumstances of men of science. If we look at the station in society occupied by the *savans* of other countries, in several of them we shall find it high, and their situations profitable. Perhaps, at the present moment, Prussia is, of all the countries in Europe, that which bestows the greatest attention and most unwearied encouragement on science. Great as are the merits of many of its philosophers, much of this support arises from the character of the reigning family, by whose enlightened policy even the most abstract sciences are fostered.

The maxim that "knowledge is power," can be perfectly comprehended by those only who are themselves well versed in science; and to the circumstance of the younger branches of the royal family of Prussia having acquired considerable knowledge in such subjects, we may attribute the great force with which that maxim is appreciated.

In France, the situation of its *savans* is highly respectable, as well as profitable. If we analyze the list of the Institute, we shall find few who do not possess titles or decorations; but as the value of such marks of royal favour must depend, in a great measure, on their frequency, I shall mention several particulars which are probably not familiar to the English reader. *

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* This analysis was made by comparing the list of the Institute, printed for that body in 1827, with the *Almanach Royale* for 1823.

	Number of the Members of the Institute of France who belong to the Legion of Honour.	Total Number of each class of the Legion of Honour.
Grand Croix -	3	80
Grand Officer -	3	160
Commandeur -	4	400
Officer - -	17	2,000
Chevalier - -	40	Not limited.

	Number of Members of the Institute decorated with the Order of St. Michel.	Total Number of that Order.
Grand Croix -	2	100
Chevalier - -	27	

Amongst the members of the Institute there are,—

Dukes - - -	2
Marquis - - -	1
Counts - - -	4
Viscounts - -	2
Barons - -	14
	23

Of these there are Peers of France 5

We might, on turning over the list of the 685 members of the Royal Society, find a greater number of peers than there are in the Institute of France; but a fairer mode of instituting the comparison, is to inquire how many titled members there are amongst those who have contributed to its *Transactions*. In 1827, there were one hundred and nine members who had contributed to the Transactions of the Royal Society; amongst these were found:—

Peer - - - -	1
Baronets - - -	5
Knights - - -	5

It should be observed, that five of these titles were the rewards of members of the medical profession, and one only, that of Sir H. Davy, could be attributed exclusively to science.

It must not be inferred that the titles of nobility in the French list, were all of them the rewards of scientific eminence; many are known to have been such ; but it would be quite sufficient for the argument to mention the names of Lagrange, Laplace, Berthollet, and Chaptal.

The estimation in which the public hold literary claims in France and England, was curiously illustrated by an incidental expression in the translation of the debates in the House of Lords, on the occasion of His Majesty's speech at the commencement of the session of 1830. The Gazette de France stated, that the address was moved by the Duc de Buccleugh, "*chef de la maison de Walter Scott.*" Had an English editor wished to particularize that nobleman, he would undoubtedly have employed the term *wealthy*, or some other of the epithets characteristic of that quality most esteemed amongst his countrymen.

If we turn, on the other hand, to the emoluments of science in France, we shall find them far exceed those in our own country. I regret much that I have mislaid a most interesting memorandum on this subject, which I made several years since : but I believe my memory on the point will not be found widely incorrect. A foreign gentleman, himself possessing no inconsiderable acquaintance with science, called on me a few years since, to present a letter of introduction. He had been but a short time in London ; and, in the course of our conversation, it appeared to me that he had imbibed very inaccurate ideas respecting our encouragement of science.

Thinking this a good opportunity of instituting a fair comparison between the emoluments of science in the two countries, I placed a sheet of paper before him, and requested him to write down the names of six Englishmen, in his opinion, best known in France for their scientific reputation. Taking another sheet of paper, I wrote upon it the names of six Frenchmen, best known in England for their scientific discoveries. We exchanged these lists, and I then requested him to place against each name (as far as he knew) the annual income of the different appointments held by that person. In the meantime, I performed the same operation on his list, against some names of which I was obliged to place a *zero*. The result of the comparison was an average of nearly L. 1200 per annum for the six French *savans* whom I had named. Of the average amount of the sums received by the English, I only remember that it was very much smaller. When we consider what a command over the necessaries and luxuries of life L. 1200 will give in France, it is underrating it to say it is equal to L. 2000 in this country.

Let us now look at the prospects of a young man at his entrance into life, who, impelled by an almost irresistible desire to devote himself to the abstruser sciences, or who, confident in the energy of youthful power, feels that the career of science is that in which his mental faculties are most fitted to achieve the reputation for which he pants. What are his prospects? Can even the glowing pencil of enthusiasm add colour to the blank before him? There are no situations in the state; there is no position in society to which hope can point, to cheer him in his laborious path. If, indeed, he belong to one of our universities, there are some few chairs in his *own* Alma Mater to which he may at some distant day pretend; but these are not numerous; and whilst the salaries attached are seldom sufficient for the sole support of the individual, they are very rarely enough for that of a family. What then can he reply to the entreaties of his friends, to betake himself to some business in which perhaps they have power to assist him, or to choose some profession in which his talents may produce for him their fair reward? If he have no fortune, the choice is taken away: he *must* give up that line of life in which his habits of thought and his ambition qualify him to succeed eminently, and he *must* choose the bar or some other profession, in which, amongst so many competitors, in spite of his great talents, he can be but moderately successful. The loss to him is great, but to the country it is greater. We thus, by a destructive misapplication of talent which our institutions create, exchange a profound philosopher for but a tolerable lawyer.

If, on the other hand, he possess some moderate fortune of his own; and, intent on the glory of an immortal name, yet not blindly ignorant of the state of science in this country, he resolves to make for that aspiration a sacrifice the greater, because he is fully aware of its extent;—if, so circumstanced, he give up a business or a profession on which he might have entered with advantage, with the hope that, when he shall have won a station high in the ranks of European science, he may a little augment his resources by some of those few employments to which science leads;—if he hope to obtain some situation, (at the Board of Longitude,* for example,) where he may be permitted to exercise the talents of a philosopher for the paltry remuneration of a clerk, he will find that other qualifications than knowledge and a love of science are necessary for its attainment. He will also find that the high and independent spirit, which usually dwells in the breast of those who are deeply versed in these pursuits, is ill adapted for such appointments; and that even if successful, he must hear many things he disapproves, and raise no voice *against* them.

Thus, then, it appears that scarcely any man can be expected to pursue abstract science unless he possess a private fortune, and unless he can resolve to give up all intention of improving it. Yet, how few thus situated are likely to undergo the labour of the acquisition; and if they do from some irresistible impulse, what inducement is there for them to deviate one step from those inquiries in which they find the greatest delight, into those which might be more immediately useful to the public?

General state of learned societies in England.

The progress of knowledge convinced the world that the system of the division of labour and of co-operation was as applicable to science, as it had been found available for the improvement of manufactures. The want of competition in science produced effects similiar to those which the same cause gives birth to in the arts. The cultivators of botany were the first to feel that the range of knowledge embraced by the Royal Society was too comprehensive to admit of sufficient attention to their favourite subject, and they established the Linnean Society. After many years, a new science arose, and the Geological Society was produced. At an another and more recent epoch, the friends of astronomy, urged by the wants of their science, united to establish the Astronomical Society. Each of these bodies found, that the attention devoted to their science by the parent establishment was insufficient for their wants, and each in succession experienced from the Royal Society the most determined opposition.

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* This body is now dissolved.

Instituted by the most enlightened philosophers, solely for the promotion of the natural sciences, that learned body justly conceived that nothing could be more likely to render these young institutions permanently successful, than discouragement and opposition at their commencement. Finding their first attempts so eminently successful, they redoubled the severity of their persecution, and the result was commensurate with their exertions, and surpassed even their wildest anticipations. The Astronomical Society became in six years known and respected throughout Europe, not from the halo of reputation which the glory of its vigorous youth had thrown around the weakness of its declining years; but from the sterling merit of "its unpretending deeds, from the sympathy it claimed and received from every practical astronomer, whose labours it relieved, and whose calculations it lightened."

But the system which worked so well is now changed, and the Zoological and Medico-Botanical Societies were established without opposition: perhaps, indeed, the total failure of the latter society is the best proof of the wisdom which guided the councils of the Royal. At present, the various societies exist with no feelings of rivalry or hostility, each pursuing its separate objects, and all uniting in deploring with filial regret, the second childhood of their common parent, and the evil councils by which that sad event has been anticipated.

It is the custom to attach certain letters to the names of those who belong to different societies, and these marks of ownership are by many considered the only valuable part of their purchase on entry. The following is a list of some of these societies. The second column gives the ready-money prices of the tail-pieces indicated in the third.

SOCIETIES.	Fees on Admission, including Composition for annual Payments.			Appended Letters.
	L.	s.	d.	
Royal Society	50	0	0	F. R. S.
Royal Society of Edinburgh	25	4	0*	F. R. S. E.
Royal Academy of Dublin	26	5	0	M. R. I. A.
Royal Society of Literature	36	15	0	F. R. S. Lit.
Antiquarian	50	8	0	F. A. S.
Linnean	36	0	0	F. L. S.
Geological	34	13	0	F. G. S.
Astronomical	25	4	0	M. A. S.
Zoological	26	5	0	F. Z. S.
Royal Institution	50	0	0	M. R. I.
Royal Asiatic	31	10	0	F. R. A. S.
Horticultural	48	6	0	F. H. S.
Medico-Botanical	21	0	0	F. M. B. S.

Thus, those who are ambitious of scientific distinction, may, according to their fancy, render their name a kind of comet, carrying with it a tail of upwards of forty letters, at the average cost of L. 10, 9s. 9¼d. per letter.

Perhaps the reader will remark, that science cannot be declining in a country which supports so many institutions for its cultivation. It is indeed creditable to us, that the greater part of these societies are maintained by the voluntary contributions of their members. But, unless the inquiries which have recently taken place in some of them should rectify the *system of management* by which several have been oppressed, it is not difficult to predict that their duration will be short. Full *publicity*, *printed statements* of *accounts*, and occasional *discussions* and inquiries at *general meetings*, are the only safeguards; and a due degree of *vigilance* should be exercised on those who *discourage* these principles. Of the Royal Society, I shall speak in a succeeding page; and I regret to add, that I might have said more. My object is to amend it; but,

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[*] The Royal Society of Edinburgh now requires, for composition in lieu of annual contributions, a sum dependent on the value of the life of the member.

like all deeply-rooted complaints, the operation which alone can contribute to its cure, is necessarily painful. Had the words of remonstrance or reproof found utterance through other channels, I had gladly been silent, content to support by my vote the reasonings of the friends of science and of the Society. But this has not been the case, and after frustrated efforts to introduce improvements, I shall now endeavour, by the force of plain, but perhaps painful truths, to direct public opinion in calling for such a reform, as shall rescue the Royal Society from contempt in our own country, from ridicule in others.

On the next five societies in the list, I shall offer no remarks. Of the Geological, I shall say a few words. It possesses all the freshness, the vigour, and the ardour of youth in the pursuit of a youthful science, and has succeeded in a most difficult experiment, that of having an oral discussion on the subject of each paper read at its meetings. To say of these discussions, that they are very entertaining, is the least part of the praise which is due to them. They are generally very instructive, and sometimes bring together isolated facts in the science which, though insignificant when separate, mutually illustrate each other, and ultimately lead to important conclusions. The continuance of these discussions evidently depends on the taste, the temper, and the good sense of the speakers. The things to be avoided are chiefly verbal criticisms—praise of each other beyond its reasonable limits, and contest for victory. This latter is, perhaps, the most important of the three, both for the interests of the society and of truth. With regard to the published volumes of their Transactions, it may be remarked, that if members were in the habit of communicating their papers to the Society in a more finished state, it would be attended with several advantages; amongst others, with that of lightening the heavy duties of the officers, which are perhaps more laborious in this Society than in most others. To court publicity in their accounts and proceedings, and to endeavour to represent all the feelings of the Society in the Council, and to avoid permanent Presidents, is a recommendation not peculiarly addressed to this Society, but would contribute to the well-being of all.

Of the Astronomical Society, which, from the nature of its pursuits, could scarcely admit of the discussions similar to those of the Geological, I shall merely observe, that I know of no secret which has caused its great success, unless it be attention to the maxims which have just been stated.

On the Zoological Society, which affords much rational amusement to the public, a few hints may at present suffice. The largeness of its income is a frightful consideration. It is too tempting as the subject for jobs, and it is too fluctuating and uncertain in its amount, not to render embarrassment in the affairs of the Society a circumstance likely to occur, without the greatest circumspection. It is most probable, from the very recent formation of this Institution, that its Officers and Council are at present all that its best friends could wish; but it is still right to mention, that in such a Society, it is essentially necessary to have men of business on the Council, as well as persons possessing extensive knowledge of its pursuits. It is more dangerous in such a Society than in any other, to pay compliments, by placing gentlemen on the Council who have not the qualifications which are requisite; a frequent change in the members of the Council is desirable, in order to find out who are the most regular attendants, and most qualified to conduct its business. Publicity in its accounts and proceedings is, from the magnitude of its funds, more essential to the Zoological than to any other society; and it is rather a fearful omen, that a check was attempted to be given to such inquiries at the last anniversary meeting. If it is to be a scientific body, the friends of science should not for an instant tolerate such attempts.

It frequently happens, that gentlemen take an active part in more than one scientific society: in that case, it may be useful to derive instruction as to their merits, by observing the success of their measures in other societies.

The Asiatic Society has, amongst other benefits, caused many valuable works to be translated, which could not have otherwise been published.

The Horticultural Society has been ridden almost to death, and is now rousing itself; but its constitution seems to have been somewhat impaired. There are hopes of its purgation, and ultimate restoration, notwithstanding a debt of L. 19,000, which the Committee of Inquiry have ascertained to exist. This, after all, will not be without its advantage to science, if it puts a stop to *house-lists, named by one or two persons*,—to making *complimentary* councillors,—and to auditing the accounts *without examining every item*, or to omitting even that form altogether.

The Medico-Botanical Society suddenly claimed the attention of the public; its pretensions were great—its assurance unbounded. It speedily became distinguished, not by its publications or discoveries, but by the number of princes it enrolled in its list. It is needless now to expose the extent of its short-lived quackery; but the evil deeds of that institution will long remain in the impression, they have contributed to confirm throughout Europe, of the character of our scientific establishments. It would be at once a judicious and a dignified course, if those lovers of science who have been so grievously deceived in this Society, were to enrol upon the latest page of its history its highest claim to public approbation, and by signing its dissolution, offer the only atonement in their power to the insulted science of their country. As with a singular inversion of principle, the society contrived to render *expulsion* * the highest *honour* it could confer; so it remains for it to exemplify, in *suicide*, the sublimest *virtue* of which it is capable.

ART. VIII.—*Experiments on the variations to which Magnets are incident when exposed to the Solar Rays.* By Professor F. ZANTEDESCHI.†

SINCE the experiments of Dr Hook and Dr Robison, it has been well known to natural philosophers, that red hot bars of iron placed at a suitable inclination in the plane of the magnetic meridian acquire a certain degree of magnetic virtue, particularly according to Professor Barlocci, (*Giornale Arcadico*, T. 122, 1829, p. 145,) when they are in the vicinity of other magnets. We know also from the experiments of Professor Configliacchi, (*Giornale di Pavia*, 1813, T. vi.) and MM. Fusinieri, Barlow, and several others, that metallic rods partially heated excite a particular influence in magnetic needles freely suspended; and, finally, the delicate experiments of M. Kupffer, and other able natural philosophers, have shown that the intensity of magnetic action is in the direct ratio of the increase of the temperature of the magnet. Hence it follows that caloric concurs in putting electricity into motion, in developing magnetism under certain favourable circumstances, and in weakening it in others, in a manner analogous to what we observe in thermo-electric crystals, as the last experiments of Mr Ritchie have shown. But none of the philosophers that I know of has directed his attention to the influence which the solar rays may exercise in the production of electro-magnetic phenomena.

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* They expelled from amongst them a gentleman, of whom it is but slight praise to say, that he is the first and most philosophical botanist of our own country, and who is admired abroad as he is respected at home. The circumstance which surprised the world was not his exit from, but his previous entrance into that Society.

† Translated from the *Bibliotheque Universelle*, November 1829, p. 193.

Since the experiments of Professor Morichini, Professor Baumgartner has observed, that iron wires polished on a part of their length are magnetized by undecomposed solar light, exhibiting a north pole on the part which is polished. The results, however, obtained by Professor Barlocci, and by Mr Christie, (*Phil. Trans.* 1828, P. 2, p. 379,) have induced me to complete an inquiry which I had undertaken some time before the Memoirs of these natural philosophers were known to me, and the sole purpose of which is to ascertain the variations to which magnets are incident under the influence of solar light. Hence it will be seen that I do not design to treat of the action of decomposed light, a subject on which the opinion of Professor Morichini, confirmed by the experiments of Mrs Somerville and those made by myself (*Bibl. Univers.* May 1829, p. 152,) appears sufficiently established.

The observations made at Paris by Cassini in 1792 on the diurnal changes in the variation of magnetic needles, those made by Watt and Christie, and many others which it is unnecessary to mention, demonstrate the influence of undecomposed solar light upon all bodies, and in a less degree on those which are not sensibly magnetic. But these experiments, while they prove the existence of a powerful action of light upon terrestrial bodies, do not permit us to see the mode in which the solar light acts in magnetic phenomena. I ought here to remark, that about the end of 1825, I had discovered that iron needles, deprived of all sensible magnetism, and suspended under a bell glass by a very fine fibre from the cocoon of the silk worm, and having one of their extremities exposed to the light of the sun concentrated by a lens, did not delay withdrawing itself from the action of the sun, and turning that extremity to the north in the plane of the magnetic meridian; but this fact was discovered and published before me by other natural philosophers; and particular circumstances prevented me from resuming this subject before last year. It is true that my principal researches had for their object the action of the solar spectrum, nevertheless I remarked, that needles of iron, which did not possess any sensible magnetism, acquired a feeble polarity, when one of their extremities was placed for some time in compound light. But, satisfied with this first observation, I abandoned the inquiry, and did not resume it till the beginning of April in the present year. As it is in a great measure the same as that of Professor Barlocci and Mr Christie, I ought first to describe the results of these two natural philosophers, who have anticipated me by the publication of their labours, satisfied in having followed the same route without being acquainted with their discoveries.

Professor Barlocci discovered that an armed natural loadstone, which could carry a weight of 1½ Roman pounds, (a Roman lb. = 339.179 grammes,) exhibited after three hours exposure to the strong light of the sun, an increase of energy equivalent to two ounces or one-sixth of a pound, and at the end of twenty-four hours the force of the loadstone was almost doubled. A second loadstone of nearly the same strength having been put into a dark place, whose temperature was equal to that of the solar rays, did not exhibit any appreciable increase of strength. Another experiment was made with a stronger magnet, which carried five pounds, five ounces, and two deniers. This magnet having been exposed to the light in a cloudy day, in which the atmosphere was charged with humidity, and when it even snowed, no sensible increase of strength was perceived; while in the two following days, during which the sky was perfectly clear, its strength increased more than double. A more lengthened exposure of the loadstone to the solar light did not produce a greater effect.

My own experiments made with every possible care confirmed these results. An artificial horse-shoe loadstone, which carried 13½ ounces, after exposure to the sun for three days, carried 3½ ounces more; and, by continuing its exposure, its power increased till it became 31 ounces. It was not possible to obtain greater strength. I could not observe any sensible modification by a dry and cloudy day. I obtained analogous results with natural loadstones of different degrees of strength. I was now desirous of ascertaining if oxidation had any influence analogous to that which I had observed in my experiments on the violet ray. Experiment proved, that, whilst by exposure to the sun, the strength increased in oxidated magnets, it diminished in those which are not so, but that this diminution became almost insensible when the loadstone was polished so as to reflect light like a mirror. A loadstone indeed, not oxidated, which carried eight ounces, being exposed for three hours to solar light, lost 2½ ounces of its strength, whilst another oxidated loadstone similarly exposed gained as much, and even more strength; but having polished the first like a mirror, I could not observe any perceptible variation, though its exposure to the sun was greatly prolonged.

Since these experiments, which I repeated several times during the most brilliant days of April and May, I have changed my method of operating. I cause the solar light to fall concentrated by means of a lens, sometimes on one pole and sometimes on the other, beginning always on the north pole; and I am convinced that the choice of one pole in place of another was not a matter of indifference. A loadstone, whether oxidated or not, whose *north pole* is exposed to the sun, *acquires strength*; if it is its *south* pole it *loses strength*. I have also discovered by experiments made successively with different loadstones, that the augmentation of force acquired in the first case is less than the loss sustained in the second, and that the variations are more considerable in oxidated loadstones than in those which are not so. In more than 60 experiments indeed, it was from 1 to 2 and to 8¾ ounces, whilst the diminution in the corresponding case was from 3½ to 5, and to 5½ ounces.

I have observed oxidated magnets acquire an energy double of that which they had previously, which did not take place with those whose surface was clear.

Finally, I satisfied myself that cooling was a circumstance favourable to the increase of magnetism. The loss of force, indeed, which a magnet sustained whose south pole had been exposed to the sun, diminished when this exposure ceased. The increase obtained by the one whose north pole had been exposed, augmented on the contrary, in the same circumstances.

I ought not to conceal that I often encountered anomalies, of which I could not discover the cause. Magnets are a kind of Proteuses, which transform themselves under the eyes even of the most attentive observer. I trust, therefore, that philosophers who repeat my experiments, will not accuse me of inaccuracy on this subject.

A fact which surprised me extremely, and which I should still have doubted, if I had not reproduced it several times before intelligent persons, is, that in days when the sun was slightly covered with an unequal veil, the south pole submitted to the action of concentrated solar light manifested an increase of energy, whilst the north pole exhibited a diminution. It should be remarked, that in the first experiment, it was the south pole which I first submitted to the concentrated light. On the day following, which was the 4th June, I resumed my experiments at 2^h P. M. Till half-past four, the space of time during which the sun's light was very clear, I exposed alternately the poles of several magnets, and I saw reproduced the effects which I have above described, viz. an increase of strength by the exposure of the north pole, and a diminution by that of the south pole, even when I began by exposing this last pole. But after half-past four, the sun being covered with a very thin veil, the same experiments continued presented inverse phenomena, that is the same as those which I observed the day before, during which the sun was slightly covered. The same experiments reported by other persons have clearly demonstrated that these were constantly the phenomena. I freely confess that I was astonished at this contrast, and I could not assign any cause for it but by supposing that light presented a *negative polarity*, the inverse of that of the strata of vapours which float in the atmosphere, such as is observed in the ordinary phenomena of polarisation, according to the fine discoveries of Brewster and Arago upon paraselenæ.*

It will perhaps be objected, that in all these experiments the action of caloric is combined with that of light, so that the final effect is due either to the isolated influence of one of these agents or to the combined influence of both. I feel all the force of this objection, but, as I have said above, caloric acts in general as a cause which diminishes the magnetic intensity. Besides, I have recurred to direct experiments which prove that, in the phenomena described, it has not acted otherwise. If we heat a piece of brick, but not so as to become luminous, and if we bring it near one of the poles of a magnet, we shall find that this magnet will no longer carry the same weight as before. The phenomenon in question, therefore, can only be ascribed to light.

Hitherto my manner of experimenting is that of Professor Barlocci, partly modified. That which I am going to describe is imitated from Mr Christie. This able natural philosopher informs us that direct solar light, as well as a plate of copper placed in the neighbourhood, diminishes the arcs of oscillation of a moveable magnetic needle. I tried at several times to repeat the experiments of the English natural philosopher with needles three inches long, but I could not obtain satisfactory results, as was seen by Professor Configliacchi, who was so kind as to assist me in this inquiry.

In consequence of this I had a needle made a Paris foot in length, and having repeated the experiment of Christie in very clear days, I could no longer doubt the accuracy of his results. In the shade, indeed, when this needle was drawn from its position of equilibrium through an arch of 90°, it performed in 30″ four oscillations, the last of which had a semi-amplitude of 70°. When exposed to the solar rays, it performed in the same time, and under the same circumstances, four oscillations, the last of which had only a semi-amplitude of 60°. I obtained more marked effects by causing the needle to perform 6, 8, 12, and 14 oscillations. I next tried if I could discover the law which I had formerly observed relative to the poles, that is, if, by exposing to the sun the north pole of the needle, I should obtain a greater number of oscillations and a less amplitude than by exposing the south pole. A series of experiments, repeated more than thirty times, has demonstrated to me the existence of this law. I consider it superfluous to insert the three or four tables which contain my results. I shall merely observe, for those who desire to repeat these experiments, that when I expose to the sun the north pole, the semi-amplitude of the last oscillation has 6° less than that of the first, while, by exposing the south pole, this last oscillation became greater than the first.

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* We do not understand what the author means by this reference.—Ed.

It will be sufficient to add, that in days slightly cloudy the results were inverse, as happened in the other experiments, and that the diminution of temperature augmented the intensity of the directive force.

These experiments, though very delicate, have inspired me with great confidence, both on account of the regularity of the effects obtained, and on account of the manner in which they were made. I might still quote other facts which struck me in the course of my observations in June, and which tend to confirm what has been said above on the inverse action of heat and light; but as I propose to treat the subject at greater length, I defer their publication to another opportunity.

PAVIA, *July* 4, 1829.

ART. IX.—*Inquiry into the circumstances under which the Remains of some Fossil Animals were accumulated in the volcanic soil of the Velay, in France.* By S. HIBBERT, M. D., F. R. S. E., &c. &c. Communicated by the Author.

IN the memoir, by M. Bertrand de Doue, of which a translation was given in the last number of this *Journal*, concerning the bones of the Hyena and other animals which were imbedded in the tufa of Saint-Privat, an allusion was made by him to another discovery by M. Felix Robert, an intelligent naturalist of Le Puy, of the fossil remains of animals of the Bos genus, and of Cervi of a very large size, which were found near a basaltic plateau to the north of Polignac; the accumulation of which, according to the writer quoted, is due to circumstances very different from those which were the subject of his memoir. A very satisfactory inquiry into the circumstances under which these animals were discovered may be expected from the very able naturalists of this vicinity. The site was pointed out to me by M. Robert, and in giving the result of my own examination, I must add, that the accumulation of these remains is most difficult to be explained, except in connection with the more general geological history of the Velay, of which I shall attempt a very faint sketch;—professing at the same time my acknowledgment for the assistance which I have received in drawing it up, from the masterly treatise on the rocks of this province by M. Bertrand de Doue.

The lowest exposed rocks of the district of the Velay consist of granite, which is associated in a few places with such primary strata as gneiss or mica slate. The granite is, in the vicinity of Le Puy, surmounted by secondary strata, which are probably those of the *quader sandstein*. Long after the period of this deposit the vallies of this portion of France became subject to a new change. They exhibited a series of lakes flowing the one after the other along the actual course of the Loire, of which the basin of Le Puy was the most elevated. The effect of this was a tertiary calcareous deposit, comprising sandy clays, potter's clay, marly and gypseous beds, &c.; and as new races of vegetables and animals were then called into existence, remains of gramineous and other vegetables have been discovered in the deposit, along with such fossil shells as Lymneæ, Cyclostomæ, Bulimi, Planorbes, and Gyrogonites, and such large mammiferous animals as the Palæotherium and the Anthracotherium.

That the lake of Le Puy existed long, is evident by the thickness of its deposit, which attains the depth of near four hundred and fifty feet; but that its confines continued during the whole time to be lined with forests which gave shelter to vertebral animals, does not appear. Some great catastrophe seems to have occurred, which, at least in the district of the Velay, was incompatible with the existence of organic beings. This is shown in the remarkable system of beds discoverable at the ravine Des Brus, near Le Puy: at this place there are at least twelve beds of calcareous and argillaceous marls, severally varying from about two inches and a half to ten feet in thickness, which exhibit alternations, several times repeated, of beds with fresh water shells of different kinds, and with beds that show extremely few or none of these remains; while the whole of this upper deposit exhibits the absence, or at least the extreme rarity of remains or impressions of the vegetable kingdom. —From this appearance M. Bertrand de Doue has made many important deductions.

Such is a feeble sketch of the geological history of the Velay, which immediately preceded a new order and system of nature; which new order was first characterized by a cessation of the cause, whatever it was, that induced the calcareous and argillaceous fresh water deposit, which had before subsisted.

When the surface of the earth had become tranquillized, a new vegetable and animal creation appears to have taken place, to supply the deficiency which had resulted from prior changes. The first vegetable deposit which ensued, is perhaps one of the most interesting which is to be found in the whole course of our geological researches. This is the Brown Coal deposit of the German geologists, which, as a formation, is the best studied in the neighbourhood of the Lower Rhine. It is, however, sufficiently well marked in the Velay, where it exhibits, as in other districts of Europe, one common character.

This deposit, in its relations, intimates the existence of such forests as were calculated for a state of the country, where the waters of its ancient lakes were in a slow and gradual state of drainage. In the time when the brown coal of the Velay was formed, the waters of the expansions of the Loire had commenced the process of drainage; they had readily deepened for themselves a passage through the soft calcareous marl, leaving on the sides of the basin of Le Puy much wet ground. In many convenient declivities, therefore, such trees as were calculated for a marshy state of the soil rose into existence, such as the birch, the willow or the alder, and along with them fresh water fish, frogs, lizards, and numerous insects. These are accordingly the organic remains indicative of the brown coal formation, which we trace in many parts of Europe; and as the marshy state of the country, which it intimates, was calculated for particular races of animals, we accordingly find, as it can be distinctly shown on the banks of the Rhine, that several mammiferous tribes, some of which are now extinct, date their existence from the commencement of this interesting formation.

During the time that the early forests of this new state of our globe subsisted, of which this and other similar deposits in Europe give so decided an intimation, the process of degradation was rapidly proceeding; and as the debris of these marshy forests, along with the disintegrated materials of the rocks of the Velay, would be carried into low levels, we accordingly find in the deposits of the brown coal formation, various alternations of earthy and vegetable matter. Some of these ancient forests appear in time to have been obliterated; owing, probably, to the too rapid progress of disintegration which was induced, by which new lands were rapidly formed. Thus, at Roche-Lambert, near St Paulien, alternations of lignite and earthy deposits are surmounted by a bed of whitish micaceous sand about fifty feet thick, mixed with clay and attrited fragments of quartz and felspar; and at Aubepin the deposit is terminated by a bed, a few inches thick, of quartzose sand, coloured yellow by the hydrate of iron. Such forests, however, as continued to exist, were the haunts of animals of the Bos kind, and of various species of Cervi, some of a large size, and (as recent discoveries have proved) of the Rhinoceros leptorhinus of Italy, as well as of the Hyæna spelæa. Remains of the human race have not yet been found in the Velay; though such a discovery, seeing that it has been proved in the south of France that man was a contemporary of different extinct animals, is by no means improbable.

The forests and marshes of France had for some time been peopled with various races of animals, when convulsions appear to have shaken the solid rocks of Europe to their very foundation. The Velay partook deeply of the commotion; a system of volcanoes, tremendous in its nature and effects, bursting forth from the high lands from which the Loire takes its rise, and extending, for the most part, in a direction east of the river along the limits of the fresh water basin of Le Puy. Some few eruptions took place from beneath the fresh water deposit itself. The volcanic materials which were ejected consisted of trachytic felspar and of basalt, the latter substance being the most abundant. Successive torrents of lava, ejections of scoriæ, and deposits of tufa, thus spread themselves over the country, filling up declivities, and, in coating the surface of the calcareous deposit of a preceding state of the earth, protected much of it from the further process of disintegration.

But extensive as these eruptions were, we do not find that they put a stop to the vegetation of the country. Thus, at Collet, Ronzal and other sites, strata of the brown coal deposit, consisting of black carboniferous clays containing vegetable remains and accompanied with ferruginous sands, alternate with rolled masses of trachyte, phonolite, basalt, or volcanic cinders. The remains also of many large animals which have been discovered appear in an intermediary period of these convulsions, under circumstances which demand much explanation in order to be understood.

It would appear that in an advanced epoch of these volcanic eruptions, after the waters of the Loire, in hollowing out a course for themselves through the tertiary strata, had sunk nearly to their present level, that the stream was dammed up at the narrow gorge of Chamelieres by an eruption of an immense dike or mass of phonolitic lava; similar impediments to the outlet and drainage of the vallies occurring even in other sites, though in a less degree. By the inundation which thus took place, two expansions or lakes were formed, into which volcanic cinders and scoriæ, fragments of trachyte and basalt, as well as the disintegrated masses of primary rocks were washed, which were again mingled with the different products of the calcareous deposits which had previously occupied the basin. And hence, nothing can well exceed the variety of earthy mixtures which were thereby induced. In some places the mechanical force of the water and calcareous infiltrations has formed the deposit, for a considerable space, into a compact brecciated mass, rivalling the hardness and massiveness of solid lava; in other spots, by the prevalence of finer products, a regular stratification has ensued;—elsewhere, by the presence of iron in different states of oxidation acting on the finer materials of scoriæ or cinders, a mass has been induced, beautifully variegated in colour, to which the name has been given of ribboned breccia. Occasionally, where there has been no play of infiltration, we find the mass to be uncemented, loose, and earthy. A last variety consists of marly or argillaceous fragments conjoined and more or less indurated, which are varied by the presence of rolled or angular fragments of granite, small masses of olivine, remains of vegetables, or geodes of the hydrate of iron.

But other effects necessarily resulted from the damming up of the course of the Loire. The waters, which, by the filling up of their beds, were constrained to maintain a higher level, had this level again so much increased by the volcanic products washed into them as to fill the high lateral vallies. During this general overflow, therefore, we may turn our attention to the site of a lateral valley situated between St Paulien and the castle of Polignac, the tertiary deposit of which is surmounted by a mass of volcanic debris, consisting of large fragments of basalt mixed with scoriæ and ashes which were washed into it from an extensive basaltic plateau. The larger fragments, some of which are several tons weight, show, in many places, by the rounding of their angles, marks of their having been exposed to the abrading action of fierce torrents; they are also frequently united by a muddy and little coherent paste, consisting of clay and sand, the result of comminuted or decomposed materials. This stratum is indicative of the period when the adjoining volcanoes were in activity, and when, from the sudden obstacles opposed to the discharge of the waters, deluges were in full force, being of sufficient activity to wash from the neighbouring volcanic plateau masses of an immense size, so as to form one chaotic mass. In process of time, however, when the Loire, which had attained the full height of its increased level, had forced for itself an emission by some new course, or had perhaps deepened for itself a passage through its old gorge, nature was more composed; which tranquil state is indicated by the superior strata, near Cussac, which surmount the mass of huge fragments, and which consist of the finer materials of sand and clay disposed in regular strata, and often so consolidated as to resemble a soft sandstone. During this last mentioned state of the site of Cussac, the banks of the lateral branch of the lake, which once filled the valley, and which indicate a high level of waters, were evidently frequented by animals of the Bos genus, as well as by stags of a gigantic size, the remains of which have been recently found imbedded in the last mentioned strata. But although the evidence is irresistible that they pastured here during some short intervals when the volcanic focus was quiescent, the circumstances which led to their inhumation can only form the subject of conjecture, regarding which what I have to say will be brief.

It is manifest, from the very large quantity of bones which the rains have annually washed out of this deposit of sand and clay, that a herd or more of graminiverous animals have been here entombed, some entire skeletons of which have been found by the industry of M. Felix Robert, on whose estate they occur. It is, therefore, by no means improbable, that these animals had met with some accidental fate; probably from the sudden or instantaneous rise of the lake above the level which it had assumed. This is a circumstance the most likely to happen from the continued activity of the volcanoes long after this period; whence new impediments would be likely to occur, to prevent the discharge of the waters. Thus, it might arise from some outlet being again dammed up, or from the level of the lake being suddenly elevated, owing to new ejected matters being added to the former contents of its basin. Or, lastly, the animals might have met with a sudden fate independently of volcanic causes; as by some land-slip causing a further stoppage, or filling up of the basin, and so inducing a new flood by which they were drowned. Any of these inferences are easily suggested by the geological circumstances that connect themselves with the inhumation of these animals; while that of their being destroyed by an immense wave indicative of the general deluge, which in its frightful progress had swept before it immense fragments of rocks, and dispersed them over distant planes, is forbidden by the fact, that not a single stone or boulder has hitherto been found in the district of the Velay, which cannot be traced to adjoining heights.

The remaining portion of the volcanic history of the Velay, has been alluded to in the last number of this *Journal*, in the description which was given of the circumstances under which the remains of the hyena and other animals were discovered. The river Allier runs for a distance of nine leagues, a course not very far from parallel with the Loire, and from the mountains of this intermediate space, numerous flows of lava issuing from volcanic mouths may be traced; which, from their relations of superposition, and from the nature of the products ejected, have evidently belonged to races the most recent of the long period during which subterranean fires have ravaged the soil of the Velay. In this district, at the village of Saint-Privat, I recently discovered the bones of fossil animals imbedded in a volcanic tufa, and covered over by a subsequent flow of basaltic lava; while the excavations since conducted by M. Bertrand de Doue, to whom I pointed out the site, have showed, that the tufa had afforded burrows for hyenas, who had here retired with the spoils of the animals upon whose carcases they had fed.

Either contemporary with the last mentioned volcanoes, or not long after them, it is probable that the district of the Velay became the abode of man. But for the verification of this supposition, we must wait for such interesting discoveries of the bones of the human race being mingled with those of extinct animals, as have been made in the caves of the south of France.

The last eruptions which lightened up the volcanic regions of France were probably in the district adjoining that of the Velay, named the Viverais. Sidonius Apollinaris, who lived in the fifth century, has adverted to an eruption of his own time, as " one in which the earthquakes demolished the walls of Vienne; when the mountains opened and vomited forth torrents of inflamed materials; and when the wild beasts, driven from the woods by fire and terror, repaired into the town and made extensive ravages."

Traces of this last eruption ought to be discovered. They may, perhaps, be identified in a volcanic hill which I crossed between Thueys and Montpezat, where the flows of the lava show a freshness that is not to be exceeded by any of the volcanoes of the continent, those of Italy excepted. To quote the words of a companion of my journey, " waves of lava still appeared without a blade of grass upon them, as if they had flowed down the hill not a week before, or as if they were scarcely cold."

ART. X.—*Account of Experiments on the Elastic force of Steam up to twenty-four Atmospheres,* made by order of the Academy of Sciences of Paris.

THE French Government having resolved to submit steam engines to examination, consulted the Academy of Sciences respecting the means which, without checking the developement of industry or the operations of commerce, might be most suitable to prevent those disastrous accidents which might arise from the explosion of steam boilers.

This important question was examined by a special commission, whose report, discussed and approved of by the Academy, was addressed to the Minister of the Interior.

Five months afterwards, on the 9th October 1823, there appeared a royal ordonnance which rendered obligatory the measures proposed by the Academy; but it was quickly seen by the engineers, who were specially charged with the execution of this ordonnance, that the wishes of the government could only be fulfilled by executing a series of difficult and expensive experiments on the elasticity of steam at very considerable temperatures. The government engaged the Academy to undertake these experiments; and a committee, consisting of MM. Prony, Arago, Ampere, Gerard and Dulong, was appointed to superintend the construction of the apparatus, and to execute the experiments.

As similar experiments had never been made on elasticities above eight atmospheres, the committee resolved to extend them to above twenty; and in performing this most arduous task, they have evinced the greatest ingenuity and practical skill in the construction of the apparatus, and the greatest address in conducting the experiments. The results which were thus obtained, cannot fail to be considered as one of the most valuable presents which science has for a long time conferred on the arts of life; and as likely to prove one of the greatest blessings to humanity. It is a reflection on England and on her successive governments, that such experiments have not been long ago made under their direction;—it is a reflection on our public institutions, which ought in matters of science to advise and stimulate the government;—it is a reflection on the characteristic benevolence of our countrymen, that means should not have been taken to avert the tremendous calamities of explosions by steam. But in proportion as it was the duty of England, where every thing but intellectual labour is carried on by steam, to have originated and completed such an inquiry; in the same proportion is it honourable to the French Government, to the Academy of Sciences, and to the eminent committee whom they appointed, to have achieved so important a work.*

Our limits will not permit us to give any account of the apparatus employed by the committee. We shall state, however, the method of measuring the temperature of the steam, and any other details of particular interest.

"The exact measure of the temperature, as the report states, presented some difficulty. The thermometer ought not to be exposed directly to the pressure of the steam; for even though it might be able to support it without being broken, it would have been necessary to take into account the effects of the compression, the estimation of which would have been sufficiently embarrassing. In order to obviate this inconvenience, there were introduced into the boiler two gun-barrels closed at one end, and drawn to a point, to afford a resistance necessary to prevent their being crushed during the experiment. The one descended nearly to the bottom of the boiler, and the other did not go farther than the one-fourth of its depth.

The thermometers were placed in the interior of these cylinders filled with mercury, the *shortest* being used to give the *temperature of the steam*, and the *longest* the *temperature* of the water. This method, the only one which is practicable in experiments of this kind, would have been very defective, if it had not been accompanied with convenient means of rendering the variations of temperature very slow. This is one of the reasons which induced us to give to the boiler and furnace greater dimensions than would otherwise have been necessary; for we are satisfied from several trials, that near the maximum, the smallest variations in the elasticity of the steam are accompanied with corresponding variations in the indication of the thermometers.

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* Such of our readers as have perused the able paper by Mr Babbage on the Decline of Science in England, printed in this number, will find in these details a most striking proof of his sentiments.

Temperature by small Cent. thermometer.	Temperature by large Cent. thermometer.	Elasticities in metres of mercury.	Elasticities in atmospheres of 0.76 metres.	Elasticities in metres of mercury at 0°.
122 °97	123.° 7	1.62916	2. 14	1. 62916
132. 58	132. 82	2.1823	2. 87	2. 1767
132. 64	133. 3	2.18726	2. 88	2. 1816
137. 70	138. 3	2.54456	3. 348	2. 5386
149. 54	149. 7	3.484	4. 584	3. 4759
151. 87	151. 9	3.69536	4. 86	3. 6868
153. 64	153. 7	3.8905	5. 12	3. 881
163. 00	163. 4	4.9489	6. 51	4. 9383
168. 40	168. 5	5.61754	7. 391	5 6054
169. 57	169. 4	5.78624	7. 613	5. 7737
171. 86	172. 34	6.167	8. 114	6. 151
180. 71	180. 7	7.51874	9. 893	7. 5001
183. 70	183. 7	8.0562	10. 6	8. 0352
186. 80	187. 1	8.72218	11. 48	8. 6995
188. 30	188. 5	8.8631	11. 66	8. 840
193. 70	193. 7	10.0254	13. 19	9. 9989
198. 55	198. 5	11.047	14. 53	11. 019
202. 00	201. 75	11.8929	15. 65	11. 862
203. 40	204. 17	12.321	16. 21	12. 2903
206. 17	206. 10	13.0211	17. 13	12. 9872
206. 40	206. 8	13.0955	17. 23	13. 061
207. 09	207. 4	13.167	17. 3	13. 1276
208. 45	208. 9	13.7204	18. 05	13. 6843
209. 10	209. 13	13.8049	18. 16	13. 769
210. 47	210. 5	14.1001	18. 55	14. 0634
215. 07	215. 3	15.5407	20. 44	15. 4995
217. 23	217. 5	16.1948	21. 31	16. 1528
218. 3	218. 4	16.4226	21. 6	16. 3816
220. 4	220. 8	17.2248	22. 66	17. 1826
223. 88	224. 15	18.2343	23. 994	18. 1894

The committee next proceed to notice the experiments previously made on the elasticity of steam, and the formulæ given to represent them. The experiments thus noticed are those of Southern, Taylor, and Ure, none of which were carried beyond eight atmospheres, and those of Arzberger, * Professor in the Polytechnic Institution of Vienna. In the experiments of the German Professor, the elasticity was measured by the effect necessary to prevent the rising of a valve furnished with a lever. He appears to have carried his experiments so high as twenty

* *Jahrbücher des k. k. polytechnisches Institutes in Wien*, T. 1, p. 144, 1819. *Polytechnisches Journal von Dingler*, T. 12, p. 17 *Bulletin des Sciences Technologiques*, T. 1, p. 123.

atmospheres, the temperature corresponding to which was 222° cent., which it will be seen corresponds to twenty-three atmospheres in the present experiments. This difference seems to have arisen from the thermometer having been exposed to the pressure of the steam, and having thence experienced a diminution of capacity.

The formulæ noticed by the committee are those of Prony, Laplace, Biot, Ivory, Roche, Auguste, Tregaskis, (see this *Journal*, No. xix. p. 68.) Crichton, Southern, Tredgold, and Coriolis.

The formula of Prony, or $z = \mu_{,} \varrho_{,}^{x} + \mu_{,,} \varrho_{,,}^{x} + \mu_{,,,} \varrho_{,,,}^{x}$ in which z is the elastic force and x the temperature of the steam, was contrived to represent Bettancourt's observations, but the length of the calculations necessary to determine the six constants μ, ϱ, &c. renders it tedious to use this method of interpolation.

M. Laplace, following the approximative law of Dalton, viz. that the elasticities of the steam increase nearly in geometrical progression, when the temperatures are in arithmetical progression, represents the elastic force by an exponential, whose exponent is developed in a parabolic series. The two first terms appeared to him sufficient, but M. Biot proved the necessity of taking a third. (See *Traité de Phys.*T. i. p. 277, 350.)

This kind of expression the committee found to be one of those which deviates most from observation, and five or six terms of the series would now be necessary, which would make the calculation interminable. The formula of Ivory, which is of the same nature, though the coefficients are obtained by another process, has the same inconvenience. At the highest temperature of the experiments of the committee, it gives an elastic force more than double of that observed.

M. Roche, professor of Mathematics in the Marine School of Artillery at Toulon, sent to the Academy in the beginning of 1828, a memoir on the law of the elastic forces of steam. It is not merely an interpolation useful to the arts which the author proposes to establish. He regards his formula as a physical law, deduced by calculation from the most general principles of the theory of vapours. The committee regard this theory as not well founded; but they find that the formula is one of those which agree best with observation, being in error only *one* degree for twenty-four atmospheres, and a *tenth* of a degree only for about two atmospheres.

Nearly about the same time, M. Auguste of Berlin, published a formula so far like the preceding that the elastic force is represented by an exponential, whose fractional exponent contains the temperature both in its numerator and denominator. This formula is $e = a \left(\frac{b}{a}\right)^{\frac{(\omega + n)t}{n(\omega + t)}}$ where e is the elasticity in metres of mercury, a the elasticity of the vapour at 0° cent. $6 = 0.76$, $n = 100$, $\omega = 266\frac{2}{3}$, and t the temperature centigrade, setting out from that of melting ice. M. Auguste establishes this formula by considerations different from those of M. Roche, and the temperatures are reckoned on the air thermometer. The formula gives the temperature for twenty-four atmospheres, 214°.37. Observation gives 224°.2 on the mercurial, or 220°.33 on the air thermometer. The difference is then about 6°, or if we calculate the elasticity for the temperature of 220° or the air thermometer, we shall find an excess of more than two metres of mercury.

The formula originally proposed by M. Tregaskis in the *Edinburgh Journal of Science*, is not found to agree with observation at high temperatures. The deviation amounts to about 2°.

Almost all the other formulæ hitherto proposed rest on the same principles, and differ only in the constants which enter into their composition. Dr Young appears to have been the first who represented the elasticities by a certain power of the temperature, augmented by a constant number. He adopted the exponent 7. Creighton took 6 to represent Ure's experiments. Southern took 5.13, and Tredgold adopted the exponent of Creighton, changing the coefficient; and lastly, M. Coriolis adopts 5.355, deduced from Dalton's experiments below 100.° This formula differs very little from that which we have employed. It agrees well with the extreme observations, and differs only two or three tenths of a degree for the intermediate ones. The formula is $e = \left(\frac{1 + 0.01878\,t}{2.878}\right)^{5.355}$

where e expresses the elasticity in atmospheres of 0^m. 76 and t the temperature in cent. degrees setting out from 0°.

We prefer, however, the formula $e = (1 + 0.7153\,t)^5$ where e is the elasticity in atmospheres of 0^m. 76, and t the temperature setting out from 100°, being taken positively above and negatively below 100, the interval of 100 being taken for unity. The only coefficient which enters into this formula was deduced from the highest term of our observations.

We have placed in the following table the values given for the principal terms of the series, by the four formulæ which deviate the least from experiment, and which are not too difficult of calculation.

Elasticity in metres of mercury at 0°.	Elasticity in atmospheres of 0m. 76.	Temperature observed.	Temperature calc. Tredgold.	Temperature calc. De Roche.	Temperature calc. Coriolis.	Temp. calc. formula of the Committee.
1.62916	2.14	123°.7	123°.54	123°.58	123°.45	122°.97
2.1816	2.8705	133 .3	133 .54	133 .43	133 .34	132 .9
3.4759	4.5735	149 .7	150 .39	150 .23	150 .3	149 .77
4.9383	6.4977	163 .4	164 .06	163 .9	164 .1	163 .47
5.6054	7.3755	168 .5	169 .07	169 .09	169 .3	168 .7
8.840	11.632	188 .5	188 .44	188 .63	189 .02	188 .6
13.061	17.185	206 .8	206 .15	207 .04	207 .48	206 .2
13.137	17.285	207 .4	206 .3	206 .94	207 .68	207 .5
14.0634	18.504	210 .5	209 .55	210 .3	211 .06	210 .8
16.3816	21.555	218 .4	216 .29	218 .01	218 .66	218 .5
18.1894	23.934	224 .15	222 .09	233 .4	224 .0	224 ,02

The following are the formulæ by which this table has been calculated.

1. *Tredgold's*, $t = 85\sqrt[6]{f} - 75$, t being the temperature in centigrade degrees setting out from 0°, and f the elasticity in centimetres of mercury.

2. *De Roche's*, $t = \dfrac{11\,(\text{Log.}\,f - \text{Log.}\,760)}{0.1644 - 0{,}03\,(\text{Log.}\,f - \text{Log.}\,760)}$ t being the temperature in centigrade degrees above 100°, and f the elasticity in millimetres of mercury. The coefficient 0.1644 is deduced from the new observations of the committee.

3. *Coriolis's*, $t = \dfrac{2.878\sqrt[5.355]{f} - 1}{0.1878}$ t being the temperature in centigrade degrees setting out from 0°, and f the elasticity in atmospheres of 0.76 metres.

4. *The formula adopted by the Committee* is $t = \frac{\sqrt[5]{f} - 1}{0.7153}$, t being the temperature in centigrade degrees setting out from 100°, and taking for unity the interval of 100°, and f the elasticity in atmospheres of 0.76 metres.

By comparing the last five columns of this table, it will be seen, that, as far as three or four atmospheres, the three first of these columns represent the observations with sufficient fidelity, but beyond this the fourth of these columns, adopted by the committee, is constantly nearer to the experimental results. The greatest difference is 0.°4, and in almost all the rest it is only 0.°1. A more considerable deviation which takes place in the two first terms is of little consequence in this part of the scale, so that the formula may be applied to the arts even in this interval. Though by the nature of the experimental process which we employed, the errors ought to be proportionally greater for low pressures, it is not probable that the formula errs in defect from this cause, for it will be perceived that for smaller pressures than one atmosphere, the divergence increases more and more, in proportion as we descend lower. Hence it appears that the employment of the formula ought to be limited to elasticities greater than one atmosphere. The formula of Tredgold may be used to 100°, or even to 140°.

Having thus obtained a very simple formula, which agrees so perfectly with observation, we may employ it in calculating the table which forms the principal object of these researches; and as the only coefficient which enters into it has been determined from the last term of the series, we cannot doubt, from its coincidence with the preceding terms, that it may be extended much farther without any notable error. We are persuaded that at 50 atmospheres the error will not exceed a degree.

The following table contains the temperatures calculated for pressures which increase by half atmospheres from one to eight, by whole atmospheres from eight to twenty-four, where the observations end; and by five atmospheres, from twenty-five to fifty, supposing that the formula extends so far.

Table of the Elastic forces of Steam, and of the corresponding temperatures.

Elasticities in Atmospheres.	Elasticities in metres of mercury at 0.°	Temperatures on the mercurial centigrade thermometer.	Pressure on a square centimetre in Kilogrammes.
1	0.7600	100°	1.033
1½	1.1400	112 .2	1.549
2	1.5200	121 .4	2.066
2½	1.9000	128 .8	2.582
3	2.280	135 .1	3.099
3½	2.66	140 .6	3.615
4	3.04	145 .4	4.132
4½	3.42	149 .06	4.648
5	3.80	153 .08	5.165
5½	4.18	156 .8	5.681
6	4.56	160 .2	6.198
6½	4.94	163 .48	6.714
7	5.32	166 .5	7.231
7½	5.70	169 .37	7.747
8	6.08	172 .1	8.264
9	6.84	177 .1	9.297
10	7.60	181 .6	10.33
11	8.36	186 .03	11.363
12	9.12	190 .0	12.396
13	9.88	193 .7	13.429
14	10.64	197 .19	14.462
15	11.40	200 .48	15.495
16	12.16	203 .60	16.528
17	12.92	206 .57	17.561
18	13.68	209 .4	18.594
19	14.44	212 .1	19.627
20	15.20	214 .7	20.660
21	15.96	217 .2	21.693
22	16.72	219 .6	22.726
23	17.48	221 .9	23.759
24	18.24	224 .2	24.792
The following numbers are calculated.			
25	19.00	226 .3	25.825

30	22.80	236 .2	30 990
35	26.60	244 .85	36.155
40	30.40	352 .55	41.320
45	34.20	259 .52	46.485
50	38.00	265 .89	51.650

The temperatures which correspond to the tensions from one to four atmospheres inclusive, have been calculated by the formula of Tredgold, which in this part of the scale accords better than the others with our observations.—Abstract, from the *Ann. de Chim.* Jan. 1830.

ART. XI.—*On Inscriptions in living Trees.* By Dr C. A. AGARDH, Professor of Economy and Botany at Lund. Translated from the Swedish by JAMES F. W. JOHNSTON, A. M.

IT is a thing worthy of remark, that the older the human race becomes, and the farther it shoots forward from its origin, the more knowledge do we derive concerning the earth's "olden time." Providence has preserved the records of the earth often in the most singular manner, so that when man has believed every source of information to be exhausted, new light has suddenly burst in upon him from an unexpected quarter to show him periods formerly unknown.

For a whole thousand years we have been accustomed to believe that no important knowledge concerning the doctrines of former times was to be obtained except from the writings of the Hebrews, Greeks, and Romans. Their literature has occupied the learned not only for its classical beauty, but as the store-house of all that we know of the older history of man. Many still live in the opinion that the few ages embraced by the history of these nations are the only ones, the transactions of which it is necessary to know. Yet they are but as fixed stars of the first magnitude in the infinity of space, beyond which the telescope shows us thousands as worthy to be known as they.

When civilization carried Europeans to India, they found there an unknown literature and unknown histories,—histories relating to mysterious and strange times, and a high though ancient learning. Long was the key to this learning sought for in vain, till when it was almost despaired of, and when the excitation of curiosity was the only result of research into the historical monuments of India: the unriddling of the Egyptian hieroglyphics was at length effected, and singularly enough whole libraries were found at the same time written in the sacred character. This circumstance the more awakens our surprise that we do not possess a single Greek or Roman manuscript more ancient than a thousand years, while in the pyramids of Egypt are found rolls of papyrus preserved not for one but for several thousands of years. Thus the papyrus sprung up on the banks of the Nile, that its imperishable leaf might convey a correspondence from the world most ancient to its newest times.

But all these documents give us only an individual history. In later times other annals have been discovered, which commemorate, not heroes, but ages,—not wars and exploits, but the revolutions of the earth. They refer to a still older time, and bear us back to the six days before man was created. It is incredible with what care the earth preserves these memorials in its bosom. Insects of a day, embalmed in amber, are preserved for thousands of years. Skeletons, which under present circumstances perish in a few decades,—Palms, whose loose stems decay more speedily than other trees,—the very Algæ, so easily destroyed by storms and seasons,—nature imbeds carefully in certain deposits, to constitute together so many hieroglyphics in that writing of which, when destroyed, they shall afterwards testify the existence: as a man who, on the eve of shipwreck, seeing death inevitable, draws up an account of his misfortune, and commits it to the same element which destroys himself, yet faithfully bears his message to the land of his home.

One would hardly suppose that inscriptions on trees, the result generally of idleness or caprice, could be ranked among historical monuments. We conceive, that, once overgrown with bark, they are for ever obliterated. But it is not so. The bark only seals it up, and hides it from the gaze of unseasonable or impertinent curiosity. While the tree exists, the inscription remains entire within, and the older it becomes, the deeper is it buried, and the more safely preserved. It is not improbable that such inscriptions may be employed as historical monuments, and though they are not so at present, they may lead us to the chronology of monuments whose age we have not hitherto attained with certainty. A couple of examples will explain my meaning.

Decandolle states, but without naming his authority, that there were found upon trees in India, inscriptions in the Portuguese tongue which had been inscribed several centuries before, when the land was in the possession of the Portuguese.* Adanson, who travelled in Senegal in 1749,† met upon the Magdalena Islands, near Cape Verd, with some baobab trees, on which were found inscribed letters of a high antiquity. Tevet had seen the same inscriptions in 1555, although they were still so legible that the names of travellers of the 14th and 15th centuries could be made out. Adanson found the trees six feet in diameter. He concluded from Tevet's statement, that in his time they were three or four feet, and he found that they could not have been under a certain size when the inscriptions were made. He knew, besides, that a baobab stem of a year old has a diameter of one to one-and a-half inches, of ten years old one foot, and of 30 years two feet. These he considered so many terms of a series, which he filled up according to the same law, and found, that a boabab tree of thirty feet in diameter would have an age of 5150 years, or little less than the age of the world itself, as deduced from the sacred writings. He adds, that he himself saw boabab trees of twenty-seven feet in diameter, which must, therefore, have an age of 4280, and which seemed in their most flourishing state.‡

This age, however immense it may appear to our imagination, on closer consideration, does not appear extravagant. Animals bear in themselves the germ of their own dissolution, or the cessation of their organic life. One of the conditions of life is, that the molecules in their solid parts continually change; but as this takes place incompletely, so that a portion always remains in the state of inefficient particles, it must happen, in a shorter or longer period, that these inefficient particles become so augmented, that the active molecules are no longer sufficient for the discharge of the vital functions,—the membranes stiffen, and circulation at length stops of itself. Animal life is thus a finished cyclus, necessarily returning to that nothing from which it sprung. Among plants, such a change has also place, but the essential difference between plants and animals lies in the production of new organs. In the animal, a new artery or new lungs are not produced in the place of those which approach their period of inefficiency, but in vegetables new vessels and new parts are continually produced instead of those which have become inactive. Between the bark and the wood is deposited every year a new layer of organs, while the inner ones gradually die, and every spring a new foliage shoots forth instead of that which fell the preceding harvest. Hence the trunks of trees are often seen entirely hollow, and yet bearing a fresh and flourishing crown, and with such decayed stems they may live for ages. The well known chestnut tree on Mount Etna called *Castagno di cento cavalli*, because a hundred horses could stand under its shade, has had from time immemorial so wide a perforation in its trunk that two carriages can be driven through it abreast, and a hut is built in the centre. It continues, however, still to be an object of admiration to the traveller.* The hollow

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* Decand. *Organogr.* p. 185. This is probably the same inscription mentioned in the sequel of this paper.

† Decandolle says 1759, which is a mistake.

‡ Adanson, *Familles des Plantes*, p. 214.

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* This is different from the account given of it by Brydone, in whose time it was divided into four large arms, the junction of which was beneath the surface—and the priest said that by actual digging he had found they *still* formed one immense trunk underneath. See Brydone's *Tour*—also *Library of Entertaining Knowledge on Forest Trees.*

Damory's oak in Dorsetshire is one of the most remarkable hollow trees, as it was also the largest oak of which mention is made. Its circumference was sixty-eight feet, and the cavity of it, which was sixteen feet long and twenty feet high, was about the time of the commonwealth used by an old man for the entertainment of travellers as an ale-house. The dreadful storm in the third year of the last century shattered this majestic tree, and in 1755 the last vestiges of it were sold for fire-wood. Thus in this hollow condition it must have continued to live for at least upwards of a century. The Boddington oak also, in the vale of Gloucester, was remarkable for the large cavity it contained. It was fifty-five feet in circumference at the base. The larger arms and branches were gone in 1783, and the hollow cavity was sixteen feet in its longest diameter, with the top formed into a regular dome; while the young twigs on the decayed top had small leaves about the size of those of the hawthorn, and an abundant crop of acorns. The stem of the same baobab tree above-mentioned serves often as a lodging to several negroe families, or as a burial place for their dead, which are there changed into mummies.

Vegetable life, therefore, is not like that of animals, limited by itself, but, on the contrary, it has in itself the germ of an unceasing duration, since its essence undergoes an unceasing renovation. The causes of vegetable death are to be found in external agents,—in the power of light to accelerate vegetation, and to change leaves into flowers,—in the opposition of gravity to the ascent of the sap, which may finally overcome its organic force,—in the predominance of the vegetable mass over the producing leaves,—and lastly, in external violence, which their fixed growth prevents vegetables from escaping as animals may. Thus it is not impossible that there may be plants in which all the parts are so adapted, that the external can predominate over the internal parts as little in old age as in youth; though in reality, a tree is nearly at all times alike young, since it lives only in its young parts. *

Nor are examples of old trees rare. The oak and linden tree may attain to an age of 600 or 900 years. † The seven large cedar trees which still remained on Lebanon in 1787, when Labillardiere observed them, and which had been previously observed and measured by Rauwolf in 1574, Thevenot 1658, Larque 1688, and Maundrell 1696, calculated from the rate of growth of such trees in Europe, must have an age between 1000 and 2000 years; and in the county of Surrey, in England, a *Taxus* is known so uncommonly thick, that it is supposed to have stood from the time of Cæsar. ‡

hollow had a door and one window, and a little labour might have converted the tree into a commodious and rather a spacious room.—TRANSLATOR.

* For more on this subject see Agardh's *Essai de reduire la Physiologie vegetale à des principes fondamentaux*, Lund. 1828.

† When old, the oak increases in size very slowly. In Holt forest Hampshire, there was an oak which, at seven feet from the ground, was thirty-four feet in circumference in 1759; and twenty years after, *the circumference had not increased half an inch.*—TRANSLATOR.

‡ A single individual of the *Ficus Indica* growing on the sea coast of Nerbudda occupies a space of 2000 feet in circumference, and can give shelter to 7000 men under its shadow. It is supposed to be the same tree described by Nearchus, in which case it cannot be less than 2500 years old. Dr Plott mentions an oak at Keicot, under the shade of which 4374 men had sufficient room to stand.—TRANSLATOR.

Two of the most remarkable trees in Europe for beauty and age, are

If, therefore, it be not improbable, that the baobab tree, the stem of which attains to a diameter of thirty feet, or perhaps more, may reach an age of upwards of 5000 years, then this giant of vegetation,—this child of the young earth,—may give us no inconsiderable aid in determining the true age of the earth's present surface.

The baobab grows only in Africa, the land of wonders and of fables, comprehending vast districts, into which neither the iron nor the civilization of Europe has ever penetrated, and bearing woods still respected by the hand of man,—still inhabited in peaceful fellowship by the elephant and the giraffe, which, following the instincts of self-preservation, have long withdrawn from the presence of their superior enemy. It is possible that baobab trees may have grown up in masses to us hitherto unknown—living monuments—older than the ruined pyramids, and the abandoned sculptures of India.

Such a land also is New Holland,—a part of the world of which we yet hardly know the sea coast. If so enormous a tree as the *Eucalyptus globulus*, or the *Eutassa heterophylla* can grow on places which Europeans possess, it is probable that still greater trees, and still older ones may be found in the wood, to which neither they, nor perhaps the Aborigines have ever forced their way.

But if such data may be of real use to science, and certain enough for building upon them conclusions regarding the minimum age of the present vegetation of the earth, the methods we possess of reckoning the age of trees ought to be placed beyond all dispute, and to receive the greatest possible precision.

These remarks serve fully to show, that it is of high interest to science to be able to calculate the age of trees, and that every aid which makes that calculation more certain is not only a gain to natural science, but may become a gain also to natural history.

If we examine the method by which Adanson obtained his result regarding the age of the baobab, we shall find that it

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the palm trees of Cordova, concerning which, my friend Dr Bowring informs me that there still exists a *moorish ballad* with a Spanish version.—Translator.

cannot be sufficiently precise, partly because there are only few known terms in the series he adopts as a formula for his calculations, and partly because even these are not determined with sufficient accuracy. Another method, leading more surely to the same end, must thus be sought after as an important pre-requisite to a scientific investigation.

Such a method has been long known in the counting of the yearly rings in dicotyledonous trees.

It has been found that in these trees a layer of wood is deposited every year between the bark and the wood, and that all these layers are distinctly separated by a deeper coloured ring. A tree's age should in this way be determinable from the number of its coloured rings. Every one knows that when a tree is sawn crosswise, such circles are observed, one beyond the other, with a margin between them. These are considered as the yearly rings, one of which is formed every year the tree lives. This opinion is old, and was adopted by Linnæus.

If we suppose a straight tall tree to be increased every year by one shoot in height, then for every new shoot there will arise a yearly ring, and the higher the tree, the less will become the yearly ring which accompanies its annual shoot. Only in the root, then, should there be as many rings as the tree has years. These rings, therefore, have been represented as hollow cones, placed one over the other, all having their base on the earth's surface, and ending at the extremity of each yearly shoot.

This opinion involves many suppositions. The most important is, that every year one, and only one, ring is deposited upon the mass of wood already formed. If this be not the case, the whole theory is groundless.

This opinion, Professor Link of Berlin, one of the first physiologists of our time, has decidedly opposed in a prize essay, delivered to the Royal Society of Gottingen, and honoured by that body with their prize. He maintains that the wood of the stem increases not only by an external layer, but by an increase in the whole body of the wood.* This opinion, which he expressed in his prize essay, has been much criticized, especially by Treviranus, and he has therefore sought to explain it in two later supplements,* in such a way as to bring it nearer to the received opinion. But even in his latest explanation, he denies that the yearly rings mark the yearly increase of the wood.† And though Sprengel has also declared himself against the opinion of Link, still in his last reply he continues to persist in it.‡

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* Link, *Anat.* Gott. pp. 152, 163.

Duhamel also, the father of vegetable physiology, does not consider the yearly rings to be formed at once, but to consist of several smaller layers deposited during the whole year, though varying with the seasons. Even Mirbel does not consider as certain the opinion that the number of rings is equal with that of the years of the tree's age. §

Should we attempt to decide this controversy on physiological grounds, that decision must depend on the theory of the growth of trees, concerning which authors are still less agreed. It is better to attempt by direct experiments, which cannot be difficult to make, to come to a sure and decisive result. These observations may be made in two ways. The one is by reckoning the number of rings in trees whose age is known,—a method which has been tried, and often cited by Linnæus and Decandolle.

This method is liable to objection from the uncertainty which will generally exist whether the tree whose age we investigate is really the one planted at a given time, or another planted in its stead at a later period. So soon as that time goes beyond one life, some uncertainty must always attend our observations, and thus the conclusion be liable to dispute.

The other method is to make an inscription in the wood of the tree, and to reckon the age of the layers that grow over it. If on examination the rings of wood deposited above an inscription should prove equal in number with the years that have elapsed since it was made, we should have obtained a fact the most conclusive of all.

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* Link, *Nachträge*, &c. Erstes heft. Götting. 1809, p. 44 folj. Zweytes heft. Gött. 1812. p. 38 folj.

† *Aber bezeichnen die Jahrringe die jährlich anwachsende Schichten? Dieses bezweifle ich gar sehr.* Zweytes heft, p. 4.

‡ Link, *Kritische bemerkung zu Sprengels Werk über den Baw und die Natur des Gewächse.* Halle, 1812.

§ *Elem. de Physiologie*, p. 108.

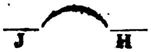
It is of importance, therefore, in this respect, to procure as many and as certain data as possible regarding such inscriptions. This physiologists have long ago acknowledged, and we find in consequence a great number of such observations. Fougeroux de Bondaroy, in the *Transactions of the French Academy* for 1777, (which I have not had the fortune to see, since they are not to be found in our library,) has inserted two memoirs, in which is a collection of such inscriptions found deep in the wood of trees. In the *Catalogue of the Library of Sir Joseph Banks*, vol. iii. p. 379, 380, and in Rausch's *Repertorium*, are found a list of writings on this subject, which also are not in the library of this university. In the *Transactions of the Swedish Academy* for 1771, Professor E. G. Lidbeck has inserted an account of some observations made by Professor Laurell. In the *Philosophical Transactions* for 1789, we remember having read some observations on this subject, by Secretary Klein in Dantzig, and Sir John Clerk, in Scotland, but which we cannot at present consult, since they are not in the library, and in Decandolle's *Organography*, are found still more observations of this kind. A revision and comparison of all these observations would undoubtedly give cause for many interesting remarks.

These inscriptions in a scientific view may be divided into two classes. The one includes those which by their growing into the tree prove the single position, that new layers of wood are deposited on the surface of the old. This fact now hardly admits of dispute, and new examples can be required only in consequence of Link's opposition. One of the observations to be found in the *Philosophical Transactions* is, that a deer's horn bound with iron cramps was found in the centre of an oak. In the same way, in the sawing of a tree in the neighbourhood of Lund, of about two feet in diameter, there was found a crooked spike sunk eight inches into the wood, without there being any external mark to show its presence. About thirty-five yearly rings lay over the spike, so that, according to the received theory, it should have begun to be covered over about thirty-five years before. This specimen is still in my possession.

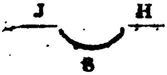
Both of these observations are more difficult to explain than the *in-growing* of an inscription; for the inscription is covered over by the layer of wood which is deposited every year between the bark and the wood, but the deer's horn and the head of the nail have naturally stood without the bark. They are explained only by the circumstances that the horn was fastened with iron cramps, and the nail was crooked. These substances, therefore, were firmly fastened to a certain point or a certain place in the interior of the tree. Whilst the tree widened, therefore, and the layers were deposited gradually further and farther out, it must have happened that these bodies at last found themselves within the layers of wood, instead of beyond the bark. Around the nail there remains still a mass of bark, which it carried along with it into the interior of the tree.

In this respect inscriptions also are not unworthy of remark, as a new proof that the new layers are deposited between the bark and the wood. But this proof is altogether superfluous. Among the inscriptions of this kind which have come to my knowledge there are one or two, concerning which, for another reason, I shall make an observation. Decandolle mentions, that [illegible], in the year 1697, found in a tree the letter H with a cross over it; and that Adami, under nineteen yearly rings, found the letters J. C. H. M. which was naturally interpreted Jesus Christus Hominum Mediator. This observation in itself is worthy of little remark; but the singularity is, that nearly the same figures and characters are found engraved and grown into two pieces of wood preserved in the Museum of Lund.

In two of these, which evidently go together, and represent both sides of the inscription found at Asum near Ofvede-kloster, is seen the following:



And in a third,



A notice in the catalogue of the museum informs us that these inscriptions belong to the time of the monks, from which we obtain some idea of their meaning, and why they should be found near a ruined cloister. Unimportant as all this may be, it has nevertheless a remarkable similarity to observations I have known made in Germany, and therefore I have not thought it proper to pass them by. I will only add to this, that, in the *Philosophical Transactions*, is an observation, that in Hans Sloane's collection was a piece of wood from the East India islands, with the following Portuguese inscription: DA BOA ORA—"(God) grant a good hour."*

It is evident that it is only by rare accident that such inscriptions can be discovered after all external traces have vanished. Since, nevertheless, we have found so many similar religious inscriptions, more of which are coming to the knowledge of scientific men every day, they must be more common in the interior of trees than is generally supposed.

The other respect in which these observations are important to science is, in case they can show that a separate layer is really deposited every year round the older wood, or that the yearly rings mark the tree's age. This they will prove so soon as we have certain and unobjectionable observations, that a writing was engraved on a certain year, and that, when after a certain number of years it was again brought to light, the number of outer rings was the same with that of the years which had elapsed. But on this point few direct observations have been made, and of those upon record only two have been made in Sweden.

Professor Laurell, who is still in lively remembrance in the University of Lund for his peculiar and often singular opinions, made an incomplete experiment of this kind. He made two inscriptions in 1748, in the wood of two separate beech trees. One of these was opened in 1764. In 1748 it was six feet six inches, and in 1764 it was found to be six feet eleven inches in circumference. The other was opened in 1756. Both contained the inscriptions within their wood. The pieces of wood on which they had been inscribed were exhibited in the Academy of Sciences in Stockholm by Professor E. G. Lidbeck, and described in their *Transactions* for 1771. The one piece which had continued to grow eight years had eight yearly rings round the inscription, and that which remained sixteen years had sixteen rings.

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* "In the *Philosophical Transactions* this is rendered *Det (Deus) bonam horam.*" I am informed by Dr Bowring, that the three words may be either an idiomatical expression having this meaning, or that it may signify simply *in bona hora*, in a lucky hour. It probably implies nearly as much as the Latin, *Quod felix faustumque sit.*

This experiment should have been very decisive; but Professor Laurell, instead of cutting the inscription through the bark, first took off the bark and then made the inscription. One of these inscriptions was long

Vivat Gustaf
skrifvit år 1748
då jag var 3 alnar
och 1 quarter tjåkk
L L *

and made a large wound, which was difficult to heal, and caused several rings to be either wanting, or to be very inconsiderable. Thus the true place of the inscription among the rings was rendered less certain. It became, therefore, a matter of considerable interest to meet with an inscription, the place of which among the rings was sufficiently manifest.

Bishop Faxe has lately sent to the Museum of Lund two pieces of wood from a tree which grew near Helsingborg, and which during the sawing and cleaving separated in such a way, that the inscription stands right on the one piece but reversed on the other, like a plate and its impression. It is as follows:—

F. M.
d. 21
J
1817

The inscription itself therefore informs us of the time when it was made. The tree was felled in 1828. There ought, therefore, to be ten complete rings, if the common opinion on this subject be correct. On examining the outer piece with a little attention, we see that the innermost ring must answer to the year 1817. Beyond this there are only nine rings; but that which is nearest to the inscription is broad and brown coloured, and must therefore denote two years, which is also manifested by the fact, that, beyond the inscription either to the right or left, it divides into two distinct layers. It is curious

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* Written in the year 1748, when I was three ells and one quarter thick.

to see the unequal breadth of the rings. The outermost, or that which lies nearest the bark, being the formation for

		1827, is	= 1.2 lines.
The second for	-	1826, is	= 0.9 ——
third	- -	1825, is	= 1.9 ——
fourth	- -	1824, is	= 1.5 ——
fifth	- -	1823, is	= 2.0 ——
sixth	- -	1822, is	= 2.0 ——
seventh	- -	1821, is	= 2.0 ——
eighth	- -	1820, is	= 2.0 ——
ninth	1819 and	1818, is	= 2.0 ——

Entire thickness = 15.5, or 1½ inches.

When we know the place where such a tree has been felled, we can compare the breadth of these yearly rings with the nature of the seasons in which they have been deposited. If we are not mistaken in the years, 1824 and 1826 should have been least favourable to vegetation. This in 1826 can be ascribed to the great drought which during that year had such an influence on the growth of plants.

Another circumstance worthy of remark is, that the tree cleft in such a way, that one-half of the inscription was found on the outer, and the other half on the inner piece of wood. This seems at first sight to confirm the opinion maintained by many celebrated physiologists,—Malpighi, Mirbel, Treviranus,—that the inner layer of the bark is changed into wood, since we might suppose that the portion of the inscription in the outer piece is that which passed through this layer. But we soon find that the mark in the outer piece is not derived merely from the inner layer, but from a portion of the whole bark, which, in consequence of the cutting, became a dead substance, and therefore did not partake in the changes of the rest of the bark, but adhered to the wood like a foreign body. The inner inscription is also of an entirely different nature from the outer one. The scar itself appears there more distinctly, and the wood around it has only become brown. But in the outer layer with the rest of the inscription the letters are indistinct, black, as if burnt, or consisting of a coal-like substance. We can, therefore, draw no other conclusion than that the new layers of wood not only *overgrow* the inscription in the wood, but that the bark also remaining attached as a dead mass was in like manner overgrown by the wood. If the outer piece be sawn across the inscription, we find traces of the writing for a considerable distance towards the bark. Near the letters it is still black and hollow; but at last there appears only a dark line, which is prolonged to the very bark, on which are some slight protuberances opposite to the inscription, as if it had still a kind of connection with the letters so far within. It may also be remarked, that in this, as in the other similar specimens I have seen, the two surfaces do not seem to have grown together. They have separated, as if of their own accord,—the reason probably of their being so often discovered. But the two surfaces are smooth and perfectly fitted to each other, so that every little prominence in the inner has a corresponding depression in the outer, as is the case with the external layer of wood and the bark.

The conclusions to be drawn from these remarks are the following.

1°. That a ring of fresh wood is deposited every year around the layers of wood which are already formed.

2°. Inscriptions may remain unchanged for ages after they are once grown over, and perish only with the substance of the tree.

3°. They do not become indistinct by length of time, but remain as distinct after many hundred years as at the end of the second.

The practical results are not less interesting.

1°. Yearly rings, if it be once established that they mark the age of the tree, may lead us to important data respecting the age of vegetation upon our earth, since there are trees which have been supposed, with probability, to have the same age as is commonly assigned to our earth. By counting the yearly rings in such trees, we shall be able, with some degree of certainty, to determine their age, and thus also the minimum age of the present surface of the earth.

2°. It is possible that by inscriptions on trees information may be handed down to posterity in a more imperishable way than by stone or brass. An oak can preserve an inscription for 500, a baobab for 4000 years or upwards.

Note.—The oak is generally supposed to live 1000 years. (Evelyn, *Silva.*) In a poem by the Emperor of China, of which a translation was published at Paris in 1770, under the title of *Eloge de Moukden*, a tree is spoken of which in that country lives more than a hundred ages, and another, which at the end of eighty ages is only in its prime. The commentators on this passage quote authority to prove that the last mentioned, " the immortal tree," is only in its prime at the age of 10,000 years. (*Chou King*, preface by M. de Guignes.) These fabulous accounts are worthy of credit, in so far as they show the existence of trees probably unknown to us, which grow to an age beyond the date of all other records.

Exclusive of physiological considerations, the only practical benefit to be derived from investigations into the age of such ancient trees would be to geology. The value of the information to this science would depend upon the nature of the formation on which they grew. Growing on primitive mountains they would be less valuable, but a tree of one, two, or three thousand years old, growing upon a newer sandstone rock, or above a coal formation, or in any similar position in regard to the later strata, would give us a minimum of age for these formations which could not fail to be highly valuable as a datum for calculating the absolute age, not merely of the local deposits but of similar deposits in the other parts of the globe. In New Holland, where such trees are found, and where strata of all relative ages have already been met with, very interesting observations of this nature may hereafter be made—and travelling geologists may, in other countries, obtain from trees much less ancient, information of no little importance regarding the date of volcanic eruptions and of the more recent alluvial deposits.—TRANSLATOR.

In connection with this subject, I add the following extract, which is both interesting in itself and as occurring in an Edinburgh weekly magazine, dated 4th July 1771.

" It is obvious, that at every place where a branch grows from the trunk of a tree, there must be a knot in the wood which will extend the whole way from the centre to the circumference, however great the diameter of the tree may be, if the branch was on it when the tree was felled. But if a branch be cut from a tree while it is growing, the stump from that time ceases to advance in size, and as the diameter of the tree continues to increase, the cortical fibres gradually cover the wood—it is soon healed up—the bark of that place becomes smooth, and the rings of wood are formed with as great regularity above the old stump as in any other part of the tree; so that when this is afterwards sawn into planks, there will be found at every such place a knot in the heart of the wood, which will extend to the same distance from the centre of the tree as was its semidiameter when the branch was cut: but beyond that to the circumference, will be uniform and free from knot or blemish of any kind whatever."

The fact here explained, is precisely similar to the *ingrowing* of insertions, and might be turned to the same account.—TRANSLATOR.

ART. XII.—*Note on a Physiological phenomenon produced by Electricity.* By Dr ET. MARIANINI, Professor of Natural Philosophy at Venice. *

IN my memoir on the shock experienced by frogs at the instant when they cease to form the arc of communication between the poles of an electrometer, (see this *Journal*, No. ii, New Series, p. 286,) I pointed out the difference which exists between the contractions produced by the immediate action of electricity upon the muscles, and which I called *idiopathic actions*, and those which proceeded from the action which electricity itself exercises on the nerves, which preside over the motions of the muscles, and which I have named *sympathetic contractions*. This difference consists in this, that the *idiopathic* contractions take place whatever be the direction in which the electric current traverses the muscles, whilst the *sympa-*

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* Translated from the *Bibl. Universelle*, December 1829, p. 287.

thetic contractions take place only when the current which traverses the nerves is in the direction of their ramification.

According to this distinction we may immediately deduce the principle, that when an electric current traverses any member of an animal, the two shocks will take place instantaneously if the electricity follows the direction of the nerves; and the idiopathic contraction will alone take place if the electricity travels in an opposite direction. The contractions ought consequently to be stronger in the first case than the second,—a result which is confirmed by experiment.

If we put the right hand in communication with the positive pole of a galvanic battery, and the left hand with the negative pole, and if the two communications are established so that the current passes with the same facility on both sides, we shall feel every time that the circuit is closed a contraction in the two arms, but it is stronger in the left arm than in the right. If we make the current pass in the opposite direction, the right arm will, on the contrary, experience a more powerful contraction than the left.

If we make one hand communicate with the positive pole, and if the negative pole is in contact with one of the feet, the electricity passes through the nerves in the direction of their ramification in the leg, and not in the arm. Consequently, the contraction is much stronger in the leg, where it is both *idiopathic* and *sympathetic*, than it is in the arm where it is only *idiopathic*. The same thing takes place when the electricity passes from the shoulder to the hand, from one foot to another, and from the thigh to the foot, &c.

This difference in the strength of the shock, according as the current goes in one direction or in the other, is greater in some individuals (particularly in paralytic persons) than in others. I have observed, in electrifying a man struck with *hemiplegia*, that, in making the current of a battery of eighty pairs pass from the hand to the shoulder, the muscles of the arm scarcely experienced a perceptible contraction, at the same place where they experienced a very powerful one if the current passed from the shoulder to the hand.

In some individuals affected with *paraplegia*, I have observed that this difference of contraction did not take place in a limb. A woman who had lost the use of her lower extremities, and of the power of extending them, in consequence of an inflammation in the spinal marrow, felt her left foot contract with more force when it communicated with the negative pole of a battery; but the right foot always contracted with the same force with whatever pole it was put in communication. This phenomenon appeared to arise from the right member having lost the power of experiencing the sympathetic shock, a loss which might be owing to a diminution of susceptibility in the nerves, to feel the effect of an electric current which traverses them in the direction of their ramification.

If we immerse a finger up to the second phalanx in a cup of water in which is plunged the positive pole of a battery of from twenty-five to thirty pairs, and if we complete the circuit by touching the negative pole with a cylinder held in the other hand and equally wetted, we shall experience in the finger a shock which extends only to the second phalanx. If we reverse the direction of the current, we shall feel the shock up to the third phalanx. What appears to me the more remarkable in this experiment, is, that by paying attention to the nature of these shocks, we shall feel that the *first* is more *external*, and accompanied with a certain sensation which is even a little painful, while the *second* is more *deep*, and is followed with no sensation at the place where the finger touches the water. I experienced so distinctly the effects of the two currents with the ring finger of the left hand, that I am confident it cannot be the effect of an illusion produced by the anticipation of it. I am of opinion then, that when the finger touches the negative pole the contraction is stronger, because the *idiopathic* and the *sympathetic* shock take place at the same time; and that when the finger is at the positive pole, the shock is weaker and accompanied with a sensation, because the portion of electricity which follows the direction of the nerves, goes in a direction opposite to their ramification. Hence, in place of producing a shock, it gives rise to a sensation,—an explanation which is conformable to what has been demonstrated in the memoir already referred to.

In seizing two metallic cylinders covered with lint, wetted, and communicating with the poles of a battery of thirty or forty pairs, moderately active, we experience, beside shocks every time that the circuit is closed, a particular sensation in the palm, which communicates with the positive pole. I have observed this sensation in a distinct manner in some individuals very sensible to the effect of electricity. They felt it to be the same with that trembling which is often felt in the hands or feet when the nerves have been for some time compressed.

It seems to me that it may be of some use to study these facts more profoundly, by submitting to the action of a voltaic current persons in a state of disease.

ART. XIII.—*On the Influence of the direction of Winds on the Electricity which accompanies the condensation of aqueous vapours in the atmosphere.* By Professor SCHUBLER, Tubingen.*

THE inquiries which I have made respecting the periodical changes of the direction of winds, and their relations to the other phenomena of our atmosphere, led me to examine, with more attention, and under this particular point of view, my former observations on the electricity of atmospheric precipitations. My principal object was to determine the electricity of the rain or snow which had fallen during thirty months. The first set of observations was made during sixteen months (from January 1805 to April 1806) at Elvanguen, and the second at Stuttgard during fourteen months, (from June 1810 to August 1811.) Ellvanguen is situated 1331 feet above the level of the sea, in 48° 57′ 25″ of north lat. and 27° 48′ east long. Stuttgard is situated 847 feet above the level of the sea, in 48° 46′ 32″ north lat. and 26° 50′ 38″ of east long. During this space of thirty months I kept account of 412 atmospheric precipitations. The examination of the first series of these observations led me to notice a certain order in the phenomena, the regularity of which became still more sensible by taking into account all the observations.

As the appreciation of the electricity of atmospheric precipitations presents several difficulties which are not met with in observations which may be made without meteorological instruments with scales more simple and more fixed, I ought to say a few words on the manner in which I obtained my results. It often happens, particularly when the rain is temporary, or proceeds from a storm, or when a very minute snow falls, that the nature of the electricity varies several times; whilst, in other circumstances, it does not vary in its intensity, its nature remaining the same during entire days. We should, therefore, obtain a very inaccurate result respecting the intensity of atmospheric electricity, if we subtracted the observed degrees of positive electricity from those of negative electricity, as is done in determining the mean temperature from the degrees of heat and cold. I therefore kept a separate account of the degrees of positive and those of negative electricity. When the opposite electricities alternated, I added separately the degrees observed and corresponding to the precipitations, whether positive or negative, and if there was a preponderance of one of the two electrical principles, I kept a proportional account. When an atmospherical precipitation gave signs only of one electricity, but without a variable intensity, I took only into account the greatest intensity which I had observed. During most rains the electrometer is indeed in a constant state of vacillation, which depends on the density, the uniformity, and the greater or less continuity with which the rains fall on the surface of the ground. When a storm approaches, the electricity sometimes becomes too intense to be measured, so that I have never pushed the observations beyond the 600dth degree of the electrometer of Volta. It is by means of this electrometer, with straws and a simple condenser, that I made all my observations, employing the same scale which I had formerly used to obtain the numerous results which I have already published.

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* *Jahrbuch der Chemie und Physik,* 1829. Heft. 3, and *Bibl. Univers.* Nov. 1829, p. 203.

The following table contains the results of my observations:

Winds.	No. of precipitations.		Ratio of positive to negative precipitations.	Mean intensity.		Mean intensity.
	Pos. elec.	Neg. elec.		Positive.	Negative.	
N.	12	11	100 : 91	131	99	116
N. E.	11	12	100 : 109	105	132	120
E.	3	5	100 : 166	15	13	13
S. E.	4	7	100 : 175	19	10	13
S.	5	13	100 : 260	26	23	24
S. W.	28	65	100 : 232	66	33	44
W.	73	106	100 : 145	75	39	43
N. W.	25	32	100 : 128	31	46	40
3. North Winds,	48	37	100 : 114	74	75	75
3 South Winds,	37	85	100 : 230	57	26	39
3. West Winds,	126	203	100 : 161	57	38	48
3. East Winds	18	24	100 : 133	71	72	72
All the Winds,	161	251	100 : 155	69	43	53

From this table we may draw the following conclusions :

1. The ratio of the positive to the negative precipitations follows a regular variation, setting out from the north or south wind, and proceeding either by the east or by the west winds.

2. By the north wind the positive precipitations are a little more frequent than the negative ones. By the south wind, on the contrary, the negative ones are more than double of the positive ones.

3. The number of negative precipitations is by the three south winds double of what it is by three north winds. The ratio is 114 : 230.

4. The east and west winds give a mean result in this respect. The former, however, approach more to those of the north, and the latter to those of the south. The electricity, indeed, is more frequently negative by the three west winds than by the three east winds in the ratio of 161 to 133.

5. The total electricity of the precipitations is more frequently negative than positive in the ratio of 155 : 100.

6. The mean intensity, on the contrary, of the positive electricity, is more considerable than that of the negative in the ratio of 69 : 43.

7. The mean intensity of the electricity, abstraction being made of its nature, is strongest by the three north winds, and particularly by north east and the north.

8. The electricity is at an average more weak by the three south winds. Its intensity by these three winds is greater than by the three north winds in the ratio of 39 : 75.

9. By the three east winds is stronger than by the three west winds in the ratio of 72 . 48.

10. The mean intensity of the electricity of all the precipitations, whether positive or negative, observed in all the directions of the wind, is almost the same as that of the electricity of the precipitations observed during the west winds alone.

11. It is during the north and the east winds that the opposite electricities are shown in the most distinct manner, and with an intensity almost equal. The west, and particularly the south, present, on the contrary, a weaker negative electricity, but a greater number of negative precipitations.

12. The greatest number of electric precipitations takes place in north winds, and the smallest number in east winds. We obtain from the mean direction of the wind during the whole of the observed precipitations 86°.9 by making use of the formula of Lambert, in which the south is marked by 0°, the west by 90°, the north by 180°, and the east by 270°. The number 86° 9′ corresponds in direction to the *west* with four degrees of declination towards the south west.

I shall now add a few words on the cause of the differences in the electricity of atmospherical precipitations according to the directions of the winds.

At the instant of the precipitation of the vapours contained in the atmosphere, positive electricity seems to be first developed, and the negative appears to proceed often from the influence of the first. The precipitations which first take place, whether storm or temporary rain and snows, are commonly positive, and are soon followed by negative electricity of nearly equal intensity. This alternation often takes place several times, while, on the other hand, we see the drops of rain, hail, hoar-frost, or flakes of snow vary every instant in their size, their density, and their continuity. At the termination, the density becomes weaker and weaker, and finally remains negative; sometimes after the storm a shower falls which possesses negative electricity.

It is not common, however, to see rains which fall regularly and continuously, show, from their commencement and during several days, only negative electricity. This fact, joined to the weak intensity which this species of electricity generally possesses, seems favourable to the opinion that it is most frequently owing to the partial evaporation which drops of rain experience during their fall. These drops form a species of evaporable base, which becomes negative by the very circumstance of the evaporation. This explanation seems to be confirmed by the observation of a fact which is owing probably to the same cause, viz. the strong electricity of the fine aqueous spray which occurs at the foot of cascades, and which is sometimes so strong near great cascades that the electrometer diverges more than 100°, as I have had occasion frequently to observe in the cataracts of Switzerland.

This explanation agrees also with the greater frequency of *negative* showers by *south* winds, and of *positive* ones by *north* winds. A current of warmer air, and one consequently more light and more elevated, in the first case ought to facilitate the evaporation of drops of rain during their fall; while, by the colder north wind, which is heavier and nearer the surface of the earth, the clouds have in general a lower position, and the evaporation of the drops of rain is less easy and almost nothing.

It follows, also, from the preceding observations, that we should be often wrong in inferring from the negative electricity of rain the negative electric state of the cloud from which the rain proceeds; for it may happen, that, coming from clouds slightly positive, it becomes negative during its fall by the partial evaporation of its drops. This I have been able to verify by direct observation in a journey which I made in the Alps. Being upon the Righi on the 10th and 11th July at a height of 5140 feet above the sea, I found, by sixteen observations made at different times of the day, that the rain which fell during two days was constantly negative; but as soon as the rain ceased a little, the clouds themselves, with which I was surrounded, were charged with positive electricity. The great intensity of the electricity, the distinct manner in which the two electric principles alternately predominate during north and east winds, seems to arise principally from the dryness which exists in the strata of air, while the whole of these winds reign in the atmosphere; to which we must add the situation of the clouds, brought by the force of these winds near the surface of the earth, and the electricity of which may then naturally exert on our instruments a perceptible influence.

ART. XIV.—*Abstracts, with occasional remarks, from a Memoir regarding the Human Bones and objects of Human Fabrication discovered in solid beds, or in alluvium, and upon the epoch of their deposition.* By M. MARCEL DE SERRES, Professor of Mineralogy and of Geology to the Faculty of Sciences of Montpellier.*

IN this article we propose to make some abstracts, of the first importance to geologists, from a very interesting memoir of M. de Serres, published in his *Geognosy of the Tertiary deposits of the South of France*, on the human bones recently discovered in the caves of France. But before giving them, it will be necessary to state certain of the writer's views on questions which have of late years distracted the minds and embarrassed the views of almost every geologist.

M. de Serres is inclined, more perhaps than any of the other of the continental geologists, to adopt the views of Dr Buckland, as explained by him in his *Reliquiæ Diluvianæ*. But he proposes such qualifications to the doctrine, as we think can scarcely be acceptable to those who wish to see in the phenomena pointed out by our English geologist, traces, not of a partial, but of an universal deluge.

After speaking of two great causes which have modified the surface of the globe, viz. the diminution or change of its temperature, and the retreat of the seas, in the last of which he leans to the well known opinion of Deluc, the writer proceeds to mention a third modifying cause, which is that of the irruptions and inundations of the sea. These he supposes to have operated in general in very restrained limits, with the exception, however, of the great inundation, the remembrance of which is preserved among all people, to which he attributes the dispersion of the boulders, often considerable, of primitive rocks, very far from their origin, and the transport of the *diluvium* upon a considerable part of the surface of the earth. But it is added by the French geologist, " this dispersion, although taking place in a manner sufficiently general, has operated, however, with the greatest irregularity, if we may judge from the unequal distribution of the mud, of the gravel, and of the boulders of rocks which are the result of it." Nor as far as we can yet learn, from perusing his views, which are exceedingly complicated, is he disposed to think that this catastrophe was in a general manner destructive to the vegetables and terrestrial animals of Europe. But we shall reserve a consideration of his views on this subject for another occasion, reserving this article of our *Journal* for the more useful purpose of giving a detail of facts, rather than solving theories. At the same time, we cannot omit this opportunity of mentioning our regret that the theoretical word *Diluvium* has been introduced in geological language, to which every geologist who has employed the term has given his own peculiar definition. M. de Serres uses it often in his memoir, but evidently with embarrassment. " The word diluvium," he observes, " which Dr Buckland was the first to propose, indicates the displaced and transported materials, or the effects of the great inundation which has submerged a portion of our continents; while by *alluvium* we have been desirous to indicate land, gained by the materials transported by rivers (terrains d'attérissemens), or land removed since historic times. But all the difficulty consists in knowing where the one stops or the other begins."

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* From the "*Geognosie des Terraines Tertiaires, &c. &c. du Midi de la France,*" recently published by M. de Serres.

These introductory observations we have conceived it proper to make, in order that our readers may correctly appreciate the weight which the author attaches to the word *Diluvium*, whenever he thinks proper to make use of the term.

The caverns in which human bones have been discovered in

France, are in number three; namely, of Bize, of Pondres, and of Souvignargues. These will be described in order.

1. *Caverns of Bize, in the Department of Aude.*

[It may be proper to premise, that of the human bones found in the cavern of Bize, M. de Serres gave an account to the Society of Natural History of Paris, of which a notice was published; but having changed his views, in consequence of a subsequent revisit to the site, the following may be considered as his corrected narrative.]

Upon visiting for the first time, the caverns containing bones at Bize, human remains were discovered fixed to the rock, and to the roof of the caverns, with other bones and land-shells; the whole cohering by means of a stalagmitic calcareous cement more or less indurated. Although these bones were in the same calcareous and stony concretions, as those of the different mammalia with which they were associated, I considered, by reason of the small number which I discovered of them, and of their state of preservation, (these bones having lost only in part their animal substance,) that they might have been entangled there in an accidental manner, and that they were not of the same date as the other remains of the animals with which they were mixed.

Since this time, M. Tournal of Narbonne, who is as zealous as he is enlightened, has discovered in the same caverns of Bize, other human bones, not only in the calcareous concretions, or the osseous breccia fixed to the roof or to the walls of these cavities, but also in the midst of the black mud, which is found the most frequently above the red mud in which bones equally exist. Along with these bones he has observed human teeth, marine and land shells of our own epoch, as well as fragments of earthen ware. The teeth which we have compared, resemble the first molar, and, like those of the other animals which are mixed with them, we perceive that they preserve their enamel. But what they have peculiar to them is, —the roots are so much changed as to adhere firmly to the tongue.

The fragments of pottery jumbled together in the same mud in which human bones, as well as the debris of terrestrial mammalia of extinct species exist, do not announce a very perfect state of the arts; for certain of them had not been baked in any furnaces constructed for this purpose. Some had been fabricated from clays, which had not previously been washed, whilst others, in short, indicated potteries still more coarse. Among the number of fragments collected by M. Tournal, some were covered over upon one of their sides with a very fine blackish dust, as if the earth of which they are wrought had been exposed to the action of fire and smoke. These earthen-vessels, totally bruised and fractured, were no less shivered than the bones with which they were mingled. The distinctness of their forms is too great to lead us to suppose, that the fragments of them had been dragged along, or conveyed from a distance by waters, which had for a long time exercised an action upon the substance which composed them. This action has been sufficiently violent to shiver these earthy vessels, but not enough to blunt their forms, or to round their angles.

The land and sea shells, which we find mixed with bones and fragments of pottery, may be all referred to species like those which we have at present. Among the former, the *Cyclostoma elegans*, the *Bulimus decollatus*, and the *Helix nemoralis* and *nitida* are the most common; as in the instance of the caverns containing bones at Lunel-Viel. These land shells still preserve, in part, their colours, as well as the marine species which are associated with them, and among which we may distinguish the *Pecten Jacobæus*, the *Mytilus edulis*, and the *Natica millepunctata.* *

The mud and the osseous breccia in which these shells, these earthen fragments, and these human bones are discovered, enclose also terrestrial mammalia, which appear to be extinct races, and others which do not at present exist in our country. Among the extinct races may be remarked a species of Cervus, of the subgenus Anoglochis, which, like the Chevreuil, has the first antler remote from the crown. And as the individuals referable to the Anoglochis, which exist in the caverns of Bize, are of a size which surpass that of the common stag, the extended wood of which have a distinct flattening, it is impossible not to regard the race as extinct, since the chevreuil is, in the present creation, the only Cervus the first antler of which is apart from the crown, or the only living Anoglochis.[*] This chevreuil, remarkable for the form of his wood, his size, and the differences which he exhibits when compared with all the species of known stags, whether living or fossil, has been named *Capreolus Tournalii* by M. de Christol, in honour of the geologist to whom the discovery of the caverns of Bize is due. With this great Capreolus, or Anoglochis, another description of them has been discovered, as well as one of the true cervus; neither of them, however, appearing to have any representative among our actual races. M. de Christol has given them the name of *Capreolus Lufroyi*, and *Cervus Reboulii*,—species which we shall describe in the work which we are preparing in concert with M. Tournal upon these caverns.

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* Although Lamarck cites this Natica as particular to the Indian ocean and to the coasts of Madagascar, it is, however, very common upon all the shores of the Mediterranean.

And again, along with these chevreuils and cervi of lost species, have been discovered the bones of bears which belong to races considered up to the present day as antediluvian, since they resemble the *Ursus Arctoideus* of M. Cuvier; a species which has only been observed in the fossil state.

Among the terrestrial mammalia which do not live any longer in our country, and the remains of which may be observed in the caverns of Bize, we shall particularly notice the Aurochs (*Bos Urus*) formerly common in the forests of Germany, and which has gradually retired into Lithuania, where it is found at the present day, according to the learned zoologist Bojanus, in some measure concentrated in the forest of Bialowiez.[†]

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[*] M. De Serres has recommended in a note, that the name of *Capreolus* be reserved for Cervi, the first antler of which is apart from the crown, and to limit that of *Cervus* to such cervi who have the first antler contiguous to the crown.

[†] De uro nostrate ejusque selecto Commentatio, H. Bojanus. (*Nova Acta Phys. Med. Acad. Nat. Curios.* tom. xiii. 2 part, page 411.)

2. Cavern of Pondres.

It was known that the caverns of Lunel-Viel inclose a great number of animals considered up to the present day as antediluvian, but we were ignorant that the caverns of Pondres and Souvignargues, situated two leagues to the north-east of the former, and which contain nearly the same species, showed at the same time, as M. de Christol has proved, human bones and earthern fragments confounded and mixed with the remains of the rhinoceros, of the bear, of the hyena, and of several other terrestrial mammalia.

These caverns, which are both opened out in the *Calcaire moellon*, are found, like those of Lunel-Viel, at but a little elevation above the Mediterranean, since M. de Christol supposes that their level is only superior to that of the sea about fifteen to eighteen metres. That of Pondres, which is only half a league from the cavern of Souvignargues, is situated upon the declivity of a small hill, where the *Calcaire moellon* is quarried for freestone. It was in working this quarry that an aperture was discovered, the height of which was about three metres by one metre in breadth, which aperture was entirely obstructed by an alluvial deposit, consisting of earth and mud, which seemed to conceal a cavern more or less spacious.

This cavity was entirely filled with alluvial mud, similar to that which filled in part the caverns of Lunel-Viel, and it was impossible to penetrate it without this substance being removed. This circumstance is the more remarkable, since it shows that the cavern of Pondres has been blocked up at one time, and not at different times, as we might be led to suppose in the case of caverns, the access of which is free and easy. This alluvial deposit or *diluvium* is less red and less tenacious than the clammy mud which holds an inferior situation at Lunel-Viel, and it acquires everywhere the solidity of tufa. There may be observed in it some pebbles of a fresh water silex, and of Jurassick limestone, along with angular and very large fragments of the *Calcaire moellon*.

The bones, which are disseminated in it at all heights, appear, however, in greater number in the middle than in the superior parts. Although this occurrence is very general, M. de Christol has, however, discovered in the highest part of the *diluvium*, that is to say, immediately under the roof, at a depth of ten to twelve centimetres at the most, a cubitus of the hyena, a cavernous bone of the aurochs, several bones of the cervus, as well as a great number of osseous fragments appearing to bear the marks of the force of teeth.

The primitive soil of the cavern, that which the *diluvium* has covered over, is formed by a sandy and tufaceous cement, proceeding probably from the decomposition of the *Calcaire moellon*. This substance is not more than thirty-two to thirty-four centimetres thick. It has become indurated by the remains of the bones and of the triturated *Album-Græcum* which it contains. Above this bed of cement, and in the diluvium, the excrements are entire and very well preserved; several of the balls of which they are composed still adhering, and being in close connection. The bones are likewise more entire, although certain of them appear to have been gnawed.

Fragments of pottery have been also discovered in the lowest as well as in the highest parts of the alluvial soil, the greatest thickness of which is about four metres. But to dissipate every doubt on this point, M. de Christol, in himself excavating the mud, collected quite at the bottom of the most inferior bed, a fragment of pottery which he was so obliging as to show us. This fragment adhered to the cement, placed above the rock which forms the floor of the cavern, being scarcely five or six centimetres above this floor. M. Dumas de Sommieres likewise observed a molar tooth, which M. de Christol has ascertained to be human, and to have come from an adult subject, which he found in the cement where exist the bones and the pounded excrements of which we have spoken. In short, in different parts of the mud other human bones have been dicovered, which resemble the phalanges of the hand, as well as a metatarsal bone, indicative of adults of a tall size.

Human bones and earthen fragments were also mixed and confounded in the Caverns of Pondres with numerous remains of terrestrial mammalia, among which were to be remarked those of the following animals:—1*st*, Of a rhinoceros more resembling the *Rhinoceros minutus* than the *Rhinoceros tichorhinus* and *leptorhinus*; 2*d*, of wild boars; 3*d*, of horses of a smaller race than the large horses of the caverns of Lunel-Viel; 4*th*, of two species of Bos, one of which was the Aurochs; 5*th*, of a description of sheep; 6*th*, of a single species of *Cervus*, probably of a cataglochis of the size of the stag; 7*th*, of a species of bear; 8*th*, of a badger; 9*th*, of the *Hyæna spelæa*, a fossil kind which approaches the most to the spotted hyena, or the hyena of the Cape; 10*th*, of *Rodentia*, the size of the hare and the rabbit.—These remains of mammalia were accompanied with the same land-shells that we found associated in the caverns of Lunel-Viel; that is to say, with the *Cyclostoma elegans*, *Bulimus decollatus*, and the *Helix rhodostoma* and *variabilis*.

3. *Cavern of Souvignargues.*

The second cavern of the environs of Sommieres (Gard), that of Souvignargues, has had formerly many openings. At the present day there only exists one of these, the others having been obstructed by parts of the cave giving way. The opening by which we penetrate it is a very irregular hole, the large diameter of which is little more than fifty centimetres. It is necessary, in this case, to crawl with some inconvenience upon the belly for a distance of about six metres, after which we are enabled to get upon our knees. This corridor, bounded on each side by cavities more or less spacious and very deep, terminates by enlarging itself. In fact, at the end of about sixty paces, it is abruptly expanded, and several very large chambers, covered over with fine stalactites, unexpectedly invite the attention of the curious. A thick, glacis-formed, stalagmitic bed covers there the diluvium; and, as it is very hard, we do not know if there exist in it bones or not.

Our operations were thus limited to the exploring of the mud which is found at the extremity of the corridor, in the place where the cavern begins to be enlarged; the mud being there completely exposed. The diluvium, the thickness of which in this part is about two metres, is red, tenacious, and apparently argillaceous. It encloses a great quantity of land-shells, like those which exist in the caverns of Pondres and Lunel-Viel; and likewise some of the *Helix nemoralis* and *algira*. Under this horizontal bed of mud containing land-shells, we observe at first, a layer of gravel about seventy centimetres thick; this bed being mixed with red clay. The gravel gradually thins off, and, in proportion to its disappearance, the bones begin to be exposed. It is in this bed that M. de Christol has detected several molar teeth of the Bos and of the cervus, an ungual phalanx of an individual of the solipeda, a molar tooth of the bear, and several human bones, as of the scapula, humerus, radius, perineum, sacrum, and two vertebræ. It is to be remarked, that under these bones there only exists a depth of twenty centimetres of *diluvium*, so that they were very close to the rock upon which this mud had been deposited. This position was one which was too important to leave undetermined, with the view of establishing, if by any circumstance the different beds of gravel had undergone any derangement; but, as we were well convinced that there was not any interruption between them, which is even the case with the superior bed, containing shells, nor any sort of dislocation, it is difficult to resist the conclusion, that whether it be the bones, or the different sorts of gravel or mud, they are found in the position, and in the situation in which they had been originally placed.

Such are the interesting circumstances under which human remains have been discovered in the south of France. M. de Serres adds in a note, that another discovery, though of fragments of pottery only, was made by M. Delanoue, in the caverns of Miremont, which contained numerous remains of the *Ursus spelæus*.

These are the chief extracts which we shall give from this important memoir. Much of the remaining part of it is dedicated to a recital of the chemical means employed to determine the relative antiquity of the bones, on the assumption, that it would bear a proportion to the carbonizable animal matter contained in them. The human bones of these caves were again compared with the oldest ones which could be collected from Gaulish sarcophagi, some of these having been interred for so long a period as fourteen or fifteen centuries. The result of these experiments was to justify the reference of the human bones found in these caves to an epoch far remoter than what could be assigned to those which were deposited not long after the Christian era. It was not, however, so easy to determine from this test the comparative age of the ossiferous deposits themselves, when considered exclusively; the greater or less abundance of the animal matter appearing to vary with the circumstances under which they were found imbedded, as for instance with their greater or less protection from the action of external agents. However, as far as could be inferred, the presumption was, that the ossiferous deposits of the caverns of Lunel-Viel and of the Hermite, in which no human remains had been detected, were the most ancient.

The fragments of pottery which were submitted to the inspection of the antiquaries of Montpellier, appeared to them to indicate the first infancy of human arts, being pronounced to belong to times anterior to the introduction of Roman inventions into Gaul. For instance, the earth of which they had been composed did not seem to have been washed before being used. Earthen vessels had been dried or hardened by the sun, or by a fire kindled on the occasion, but had not owed their baking to furnaces constructed for the purpose, which was shown by their external surfaces only having undergone the action of heat.

But to conclude.—From the circumstances under which these bones are discovered, M. de Serres has very readily arrived at two natural conclusions: 1*st*, That, since the appearance of man upon the earth, certain species of terrestrial mammalia have been completely destroyed, or at least have ceased to exist in the different parts of the globe which have been explored up to the present time; 2*d*, That the remains of our species are incontestibly mixed, and are found in the same geological circumstances as certain species of terrestrial mammalia, considered up to the present time as antediluvian, such as the bear of the caverns of Miremont and of Bize, together with the rhinoceros and the hyena of the caverns of Pondres and Souvignargues.

Besides these conclusions, there is a train of others which M. de Serres has hazarded. But, as these involve in them the details of many geological phenomena peculiar to the South of France, into which we have not space to enter, we shall postpone to another occasion our observations upon them, on account of the great length of discussion which they are calculated to involve.

We shall merely hint that the following is the state of the diluvian question:—

In the *first* place, many are inclined to think, and we believe with reason, that, in the present geological epoch, proofs are afforded of Europe having been visited by one, or even more deluges, and that, at the least, one of these was overwhelming in its effects. But it does not appear evident, that this deluge was an universal one, since no traces of it can be detected in many very extensive districts of the Continent.[*]

Secondly, Although it is probable that the deluge, or deluges with which the Continent of Europe has been occasionally visited, must have been highly destructive to many tribes of animals, yet it follows, as a corollary from the preceding conclusion, that the destruction could not have been universal; and that we must have recourse to other causes if we would explain the extinction of many ancient races of animals.

Into this question we may enter at some future period.

Art. XV.—*On the double Chlorides of Gold.* By James F. W. Johnston, A. M. Communicated by the Author.

The double chlorides of gold with sodium and potassium have been examined, the former by Figuier and Dr Thomson, the latter by Javal. The two analyses of the sodium salt agree very nearly with each other. They are as follows:

		Thomson.	Figuier.
Chloride of sodium - -		14.85	14.1
Chloride of gold	Chlorine 17.82 Gold 49.51	67.33	69.3
Water - - -		17.82	16.6
		100	100

And Dr Thomson deduces the composition to be

1 atom *bi*-chloride of gold	34	50.5
1 atom chloride of sodium	7.5	
8 water - -	9	

But this analysis bears in itself a strong argument against its correctness. The oxide of gold is as *ter*-oxide, and the chlorides by solution in water are supposed to become muriates, in consequence of the chlorine uniting with the hydrogen, and the metallic base with the oxygen of the water. When the sodium chloride of gold, therefore, is dissolved in water, three atoms of oxygen will unite with the gold to form *ter*-oxide; two of the hydrogen which is liberated will combine with the two of chlorine, forming muriatic acid; and the third will escape. Or *vice versa*, on evaporating the solution to obtain the salt in the crystalline form, there will be a redundancy of oxygen, which must in its turn fly off. But no such phenomena take place on dissolving or crystallizing the salt; it cannot, therefore, contain gold in the state of a *bi*-chloride.

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[*] Dr Hibbert, we understand, made many observations on this point of the question during his late visit to the Continent.

This difficulty was seen by Dr Thomson, and alluded to in his "*Attempt*" (vol. i. p. 447,) but, trusting to the accuracy of his analysis, he left the matter undetermined. In the eleventh volume of the *Edinburgh Transactions* (p. 23) he has returned to the subject, and endeavoured to confirm his analysis of the sodium salt by a similar analysis of the simple muriate of gold.

When the solution of gold in aqua regia is evaporated by a gentle heat, a salt is obtained in long pale yellow four-sided prisms, and in truncated octohaedrons. This is an acid salt, a compound of muriatic acid, and oxide of gold; or, as many chemists consider it, a compound of muriatic acid and chloride of gold. When these crystals are fused and further heated, they give off water, muriatic acid, and chlorine; forming a *ter*-chloride of gold which, if not sufficiently heated, may contain a portion of the acid salt,—or if heated too much, may be partly changed into *proto*-chloride, or it may be a mixture of all three.

This *ter*-chloride, according to Berzelius, consists of

		Theory.	Experiment.
1 atom gold	= 25	64.935	65.09
3 atoms chlorine	= 13.5	35.065	34.91
	38.5	100	100

If this *ter*-chloride be farther heated very gently till it ceases to give off chlorine, it is changed into a yellow *proto*-chloride, consisting, according to Berzelius, of

		Theory.	Experiment.
1 atom gold -	25	84.745	85
1 atom chlorine	4.5	15.255	15
	29.5	100	100

Water decomposes this chloride into a solution of the common muriate and metallic gold.

Such are the relations between gold and chlorine according to Berzelius. Dr Thomson, on the contrary, states, that "the muriate of gold cannot be converted into a chloride by heat; at least all my attempts to obtain a chloride by that process have ended in disappointment."*

Dr Thomson dissolved a known weight of gold in nitro-muriatic acid, evaporated to the state of a brownish-red solution; and weighed the solid mass it formed on cooling. The gold in this salt was thrown down from its solution by a plate of copper, and the copper by caustic potash. The liquid neutralized by nitric acid was thrown down with nitrate of silver. The sum of the gold in the state of peroxide, and the muriatic acid equivalent to the chlorine obtained, deducted from the weight of the saline mass, was considered as water, and the following composition made out:

$$\left.\begin{array}{lcr} \text{2 atoms muriatic acid} & = & 9.25 \\ \text{1 atom peroxide of gold} & = & 28 \\ \text{5 atoms water} \quad - & = & 5.625 \end{array}\right\} 42.875$$

There are two objections to this analysis: *First,* it will appear from a consideration of what has been stated above, that the saline mass might be a mixture, and that no characters are given by which we could determine it to be a single definite compound; and, *secondly,* the analysis itself was too complicated. It is necessary to take these objections into consideration, as the result obtained by Dr Thomson in his two analyses, if correct, would form a very remarkable anomaly among chemical combinations. Since caustic potash throws down a *ter*-oxide of gold from the muriatic solution, we should infer that salt to be a *ter*-muriate also. It is not easy to conceive, indeed, how it can be anything else.

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* *Edinburgh Transactions,* xi. p. 29.

During a residence in Stockholm towards the end of last year, I was invited by Berzelius to assist him in analyzing the chloride of gold, with the view of clearing up the anomaly which Dr Thomson's paper presented. The common muriate or chloride is not easily weighed owing to its deliquescence, nor is it easily obtained unmixed; the double salts have neither of these disadvantages; they were therefore selected for examination.

I. *Chloride of gold and sodium.*—Dr Thomson analyzed this salt by heating it alone in a green glass retort, and causing the chlorine disengaged to pass through a solution of nitrate of silver. The loss, after adding the chlorine and gold, was considered to be water. The salts analyzed in Berzelius' laboratory were introduced into a glass tube drawn to a fine point at the one end, and having a bulb blown in the middle to contain the salt. A spirit lamp was applied to the bulb, and dry hydrogen gas passed through as long as any muriatic acid was given off. The loss of weight indicated the chlorine combined with the gold,—and the water; the weight washed out by water was the alkaline salt, and the remainder was pure gold.

1°. 2.06 grammes of the salt in small yellow prisms and needles, and dried in the open air, being decomposed by hydrogen gas, and the common salt washed out, evaporated and heated to dull redness, gave loss—

being, Chlorine and water,	0.742 =	36.024 per. cent.
Gold, ————	1.02 =	49.51
Chloride of sodium,	0.298 =	14.466
	2.06	100

2°. 3.026 grammes of the same salt were mixed with six grammes anhydrous carbonate of soda, and heated to incipient redness for half an hour over a spirit lamp. The salt dissolved out by water left of gold 1.4978 grammes = 49.497 per cent. The solution saturated with nitric acid, and precipitated by nitrate of silver, gave 4.3347 grammes, fused chloride answering to 35.34 per cent. of chlorine in the salt.

The chloride of sodium obtained in the former experiment = 14.466, contains 8.835 chlorine, consequently 35.34—8.835

= 26.505 is the chlorine in combination with the gold,

And 36.024 — 26.505 = 9.519 = the water.

Therefore the composition of the salt per cent. is

Chloride of sodium,	Chlorine,	8.835*	14.466
	Sodium,	5.631	
Chloride of gold,	Chlorine,	26.505	76.006
	Gold,	49.501	
Water,			9.519

Now 8.835 × 3 = 26.505, the chlorine combined with the gold, therefore the gold is combined with three times as much chlorine as the sodium, and is a *ter*-chloride

The atomic constitution of the salt is as follows:

1 atom chloride of sodium,	= 7.5	= 14.85 per cent
1 atom ter chloride of gold,	= 38.5	= 76.23
4 atoms water,	= 4.5	= 8.92
	51.5	100

3°. From these experiments it appears that Dr Thomson has erred in both of his analyses as much as 8.835 per cent. or ⅓ of the whole chlorine combined with the gold. This wide difference between the results made me desirous of repeating these experiments on my return home before publishing those of Berzelius, more especially as the results of Dr Thomson were so nearly corroborated by the previous analysis of Figuier.

I formed, therefore, a quantity of the salt, and crystallized it from a solution containing excess of common salt. Prepared in this way, it gave me in two different analysis 16.2 per cent. of common salt, being nearly two per cent too much. I purified the salt, therefore, by the addition of more gold, and three successive crystallizations, when by spontaneous evaporation, which in this salt proceeds more slowly than in either of those hereafter described, I obtained it in large reddish yellow prisms and four-sided tables, an inch in length, and some of them near half an-inch in breadth, emitting a metallic sound when thrown upon glass.

33.038 grains of these large crystals, after drying at a heat of 150° were exposed to a current of dry hydrogen gas, and lost 11.994 grs. = 36.303 per cent. = the water and chlorine in combination with the gold.

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* These calculations are according to Berzelius's numbers.

The chloride of sodium washed out, again evaporated to dryness and heated, was perfectly white, and weighed 4.852 = 14.687 per cent.

The residual gold heated to redness weighed 16.192 = 49.01 per cent.

Again—25.781 grs. in large plates heated with carbonate of soda in the manner above described, the salt dissolved, the excess of carbonate decomposed by nitric acid, and the chlorine thrown down by nitrate of silver, gave 36.532, fused chloride of silver = 9.0299, chlorine* = 35.0255 per cent., being the whole chlorine contained by the salt.

Now the chlorine in 14.687 grs. common salt obtained in the former experiment = 8.8122 grs.

Therefore 35.0255—8.8122 = 26.2133 the chlorine in combination with the gold.

And 36.303—26.2133 = 10.0897 = water present in the salt.

The whole analysis consequently is as follows:

Chloride of sodium,	Chlorine,	8.8122	14.687
	Sodium,	5.8748	
Chloride of gold,	Chlorine,	26.2133	75.2233
	Gold,	49.01	
Water,			10.0897
			100

The result of these experiments shows, that the large crystals made use of contained upwards of one per cent. of water lodged mechanically among the plate s. The ratio of the chlorine in combination with the gold, to that united with the sodium, is the same as in the analysis of Berzelius, though not so rigidly exact as in his probably more accurate experiment.

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* This is calculated from Dr Turner's results, as detailed in his paper in the *Philosophical Transactions*. He found 100 metallic silver to be equivalent to 132.83 of fused chloride.

† This salt may be fused without undergoing any appreciable loss of chlorine, and, probably, when in large crystals this is the only way of freeing it from mechanical water. I heated 25.781 grains of large plates to the melting point, and kept it in occasional fusion for two hours, when the entire loss was only 0.29, a large portion of which was probably uncombined water.

The following table gives a view of all the analyses of this salt hitherto published.

			Theory.	Figuier.	Thomson.	Berzelius.	Johnston.
Chloride of Sodium		7.5	14.85	14.1	14.85	14.466	14.687
Chloride of gold	Chlorine	13.5	26.73	69.3	17.82	26.505	26.2133
	Gold	25.	49.5		49.51	49.501	49.01
Water		4.5	8.92	16.6	17.82	9.519	10.0897
		50.5	100	100	100	100	100

II. *Chloride of gold and potassium.*—Of this salt only one analysis, I believe, has yet been published, that of Javal.* He found it composed of

Chloride of potassium	24.26
Chloride of gold -	68.64
Water - - -	7.10
	100

This salt parts with its water much more easily than that of sodium,—it effloresces even when exposed to the air, and loses its whole water at the temperature of 212°. It crystallizes in four-sided prisms and needles, and in large brilliant thin plates, resting on their edges, and increasing upwards in the liquid, while those of the sodium salt are formed always along the bottom of the vessel. In an acid concentrated solution it forms hard prisms,—in a more neutral, fine needles. The large plates are obtained only from a solution with excess of potash, and by spontaneous evaporation. The crystals do not long retain their water. Some fine prisms picked out and preserved in a corked tube were covered with a yellow powder in a few weeks.

1°. 3.141 grammes of the salt in minute yellow needles, and previously dried by pressure upon bibulous paper, were heated on the water bath. It lost 0.255 = 8.099 per cent. Fused afterwards in a tube, it gave only a trace of aqueous vapour, showing that it contained no appreciable quantity of water.

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* *An. de Chim. et de Phys.* xvii. 337.

2°, 3.545 grammes of the dry salt heated to fusion in a current of hydrogen gas till all traces of muriatic acid ceased to be evolved, lost 0.798 = 22.51 per cent. = chlorine in combination with the gold.

The chloride of potassium being washed out, left 1.8485 of metallic gold = 52.143 per cent.

The salt therefore was composed of

	Anhydrous.	
Chloride of potassium,	25.347	23.294
Chlorine,	22.510	20.687
Gold,	52.143	47.919
Water,		8.100
	100	100

This analysis agrees very nearly with that of Javal, who calculated his salt to consist of

3 Chloride of potassium + 2 ter-chloride of gold.

This composition, calculated according to the atomic numbers of Thomson and Berzelius, gives the following proportions per cent.

	Berzelius's numbers.	Thomson's numbers.
Chloride of potassium,	27.074	27.014
Chlorine,	25.430	25.592
Gold,	47.493	47.393

These proportions differ from the experimental results by quantities far too great to be accounted errors in the analysis, as I can bear ample testimony to the care with which the above analysis was performed by Berzelius while I had the honour of assisting him. In consequence of an error in the calculation, however, by which this theoretical composition was made to differ only one per cent. from his experimental result,[*] Berzelius adopted Javal's view of the constitution of

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[*] This calculation was as follows:

Chloride of potassium,	24.77
Chlorine,	23.42
Gold,	51.81

This, it will be seen, differs from the experimental results only by one per the salt, and concluded that the sodium and potassium chlorides of gold are unlike in composition, as he had formerly found to be the case with the similar salts of rhodium.*

On my return home I was anxious to verify this analysis also, and formed therefore a portion of the salt by mixing the solutions of the simple chlorides. I obtained large, well-defined, brilliant, four-sided oblique prisms, speedily becoming opaque.

1°. Of these large prisms 50.154 grains were heated in a current of hydrogen gas, when the loss in weight was 17.4 grains = 34.693 per cent = water and chlorine.

The gold left after washing the residuum, weighed 23.5 = 46.861 per cent.

And consequently the composition of the salt was

Chloride of potassium, = 18.446
Gold, = 46.861
Chlorine and water, = 34.693

100

2°. This salt, as I have already stated, soon parts with its water, and it was by the heat of a water-bath that it was rendered anhydrous by Berzelius's experiment. But to take away all chances of error to which this method is liable, from the possible loss of chlorine and from the want of any fixed rule for determining when the water is entirely dissipated, I took 25.893 grains of the same large crystals, being all I had left, and heated them in a porcelain crucible for half-an-hour with 50 grains dry carbonate of soda.

The salt being washed out left of gold 12.15 grs. = 46.92 per cent.

The solution saturated with nitric acid and thrown down by

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cent.; and, assuming it to be correctly calculated, the composition of Javal was obviously made out. The error must have arisen entirely from the haste with which the note of the experiment given me by Berzelius was drawn up. I never thought of making the calculation myself till the impossibility of obtaining any thing like the same results in repeated analyses of the salt led me at last to examine the numbers.

* See page 22 of this present Number.

nitrate of silver gave of fused chloride, 35.666 grs. = 8.8144 chlorine = 34.0416 per cent.

Now the chlorine in 18.446 chloride of potassium, according to Thomson's numbers, is 8.7375, and 8.7375 × 4 = 34.95, which is very near the quantity of chlorine obtained. The gold, therefore, contains three times as much chlorine as the potassium, and is consequently in this salt also in the state of a *ter*-chloride.

Taking three-fourths of 34.0416, we have 25.5312 for the chlorine in combination with the gold, and 34.693 — 25.5312 = 9.1318 for the water contained in the salt. We have therefore the following constitution.

Chloride of potassium.	Chlorine, Potassium,	8.7375 9.7085	18.446
Ter-chloride of gold.	Chlorine, Gold,	25.5312 46.891	72.3922
Water,			9.1318
			100

If we calculate this composition we find the chlorides united in the proportion of atom to atom, and that the water comes nearest to four atoms, as follows.

		Theory.	Experiment.	Berzelius.
1 atom chloride of potassium, =	9.5 =	18.095	18.446	23.294
1 atom gold,	25.	47.619	46.891	47.919
3 atoms chlorine,	13.5 =	25.714	25.5312	20.687
4 atoms water,	4.5 =	8.572	9.1318	8.100
	52.5	100.	100	100

The great difference between my own results and those of Berzelius and Javal I can account for only by supposing that, my own salt being in large well-defined crystals, must have been the purer, or that there are two double chlorides of gold and potassium, of which the minute prisms analyzed in Berzelius's laboratory was a mixture. At all events, the agreement of my results with atomic numbers shows the salt I employed to have been a definite chemical compound.

III. *Chloride of Gold and Ammonium.*—Of this salt no analysis I believe has yet been published. When in Copenhagen Dr Forchammer informed me that he had made some experiments upon it, and found, that, when decomposed by a red heat, it left 48.1 per cent. of gold, and that it contained about 13 per cent. of water. These quantities, deduced from incomplete experiments, differ materially from my results as stated beneath.

This salt is easily formed. If neutral solutions of chloride of gold and sal-ammoniac be mixed and set aside for spontaneous evaporation, the salt speedily deposits itself in small golden yellow prisms and needles, or in large right prisms with rectangular terminations, and often with two of the opposite solid angles replaced so as to form right six-sided prisms. If the solution contain excess of sal-ammoniac, it forms large plates of a beautiful golden yellow colour and pearly lustre, increasing upwards, and presenting in their outline the shape of a large flat prism with low pyramidal terminations. When removed from the mother liquor, the prisms are yellow and transparent. Pressed between folds of bibulous paper, they retain their transparency for a short time—but exposure to the air for a few minutes—a slight warming of the paper—or contact with the fingers, renders them opaque. By exposure to the air for a length of time, it is gradually decomposed, and if in thin scales, dissipated, leaving a black stain. When heated in a water bath, it gives off all its water and undergoes a kind of semifusion, which causes the crystals or their powder to cohere. The colour is deepened also, but becomes again pale yellow on cooling. At a higher temperature it melts into a reddish liquid, and is decomposed, giving off chlorine and sal-ammoniac, and leaving metallic gold. This decomposition takes place so easily, that by a careful regulation of the heat, both the ammonia and the chlorine may be driven off without fusing the salt, and the metallic gold obtained in the form of the original crystal—showing that during this decomposition no change takes place in the relative position of the atoms in the interior of the crystal. A similar fact has lately been observed in regard to the crystals of nitrate of silver.

The water or ammonia or both vary in this salt so much, that though I have made ten or twelve analyses of it, I have never obtained the same per centage of the constituents in any two experiments. The water varies from 4 to 6 per cent. according as the salt has been more or less exposed to the atmosphere or other causes, so that the salt in the state of crystals, from the uncertainty of their state of dryness, cannot be employed to ascertain the per centage, nor can we depend upon their retaining all the chlorine when dried in a water bath, as the odour of this gas is developed at a very moderate temperature. The volatile nature of the alkali in this salt is another obstacle in the way of a rigorous analysis, as it is not easy to collect and weigh it. The following experiments, however, are sufficient, I think, to determine its constitution to be similar to that of the two salts above described.

1°. The mean of five experiments gives for the water driven off by the heat of a water bath 4.7 per cent.

2°. A portion of the salt decomposed by heating with carbonate of soda; and the solution afterwards saturated with nitric acid, and thrown down by nitrate of silver, gave of gold and chlorine in the following proportions:—

Gold, 7.52 = 1 atom.
Chlor. 5.477 = 4.04 atoms.

The chlorine and the gold have therefore in this salt the same ratio as in the other double salts.

3°. 32.467 grains of large distinct crystals dried in the air, and become opaque, were heated in a stream of hydrogen gas. Water passed over at first, mixed with a minute portion of acid. This ceased before the salt fused; it was therefore allowed to cool, and weighed. The loss was 1.8 grains = 5.54 per cent. Heated again till all was driven off there remained 53.22 per cent. of gold, the additional loss being 13.387 = 41.23 per cent. which ought to be something less than the full weight of the chlorine and ammonium.

Now 3 atoms chlorine = 38.738 per cent.

And 41.23 — 28.718 = 12.492, which is less than the whole weight of sal-ammoniac. Therefore we have the composition.

Gold = 53.22 per cent.
Chlorine = 28.718 ———
Sal ammoniac = 12.492 + ———
Water = 5.54 — ———

The sal-ammoniac is here too little, and the gold too much. I have tried to collect the sal-ammoniac and weigh it, but the chlorine carries off a portion of it, even when made to pass through water, so that I have contented myself with inferring its amount; in which, after the chlorine is determined, there cannot be any great error.

4°. The mean of the last compared with five other experiments, gives for the gold in the salt 52.66 per cent.

These experimental results agree very nearly with the following constitution:—

				Theory.		Experiment.
1 atom gold	=	25	=	52.632	=	52.66
4 atoms chlorine	=	18	=	37.894	=	38.33
1 atom ammonium*	=	2.25	=	4.737	=	4.3
2 atoms water	=	2.25	=	4.737	=	4.7
		47.5		100		100

IV. *Red double chlorides of Gold.*—The salts of sodium and ammonium above described dissolve in nitro-muriatic acid with effervescence and evolution of deutoxide of azote and chlorine, and give solutions varying in colour from a reddish-yellow to a deep blood-red. On cooling, these solutions deposit yellow prisms resembling in appearance the original salt, but having the property of becoming deep red when heated to about 300°. If the solution be evaporated to dryness, a blood-red mass is obtained, deepening often in the ammonium salt to a dark purple,† which on cooling attracts moisture with great avidity, becomes of a dirty brown, and finally of a yellow colour,—and resolves itself at last into a yellow liquid. This liquid, concentrated by heat, becomes again deep red, and concretes into a mass of deep red prisms, which change, as before, in a cool and moist atmosphere. In this state the salt is partially decomposed by alcohol into the alkaline chloride which remains undissolved, and a greenish[*] yellow sediment, showing to the microscope a mixture of brilliant particles of gold, and changed instantly by a drop of muriatic acid into pure metallic gold of the common appearance. The alcohol also dissolves a portion becoming deep red, and giving by evaporation deliquescent reddish-yellow crystals in four-sided prisms, becoming deep red by a heat of 212°. If the heat be raised a little higher, the salt fuses, dries, and the microscope shows the crystals to have now the form of very minute cubes. Alcohol has on these cubical crystals the same effect as at first, decomposing one portion and dissolving another, which gives again yellow prisms with a shade of red or brown, succeeded on the application of heat by dark red cubes. Water dissolves the red mass with residue of a little metallic gold, the result, probably, of previous decomposition. This solution gives a mass of red prisms, more deliquescent in the ammonium than in the sodium salts, and changed by a continuance of heat into red cubes. If the salt be previously in solution from the presence of a small quantity of moisture, alcohol does not decompose it. Like the yellow salts it is decomposed by caustic ammonia, which throws down a light brown precipitate.

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* 1 atom ammonium = 1 atom-ammonia + 1 atom hydrogen. The constitution of this salt requires I think, that we should consider sal-ammoniac as a chloride; otherwise we should have one atom of a chloride united to one atom of a muriate, a kind of compound not hitherto recognized.

† This very deep colour is generally attended by a partial decomposition.

By a similar process we should obtain a red potassium salt also. The potassium salt above analyzed dissolves like the others in aqua regia, and gives a red solution; but evaporation to dryness did not give me a red mass, nor did the yellow prisms obtained become red by an elevation of temperature. Its formation in this case may be dependent upon circumstances of which I am not at present aware.

Berzelius[†] has described similar red salts of iridium, osmium, rhodium, and palladium, formed generally after the same manner. Magnus[‡] has also formed a like class of red platinum salts, so that those now indicated were alone wanting to fill up this entire family of analogous compounds.

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[*] Berzelius states, that if the solution of chloride of palladium in caustic ammonia be evaporated to dryness and treated with water, a greenish-yellow powder remains, consisting of one atom of the chloride, and one atom of ammonia. Magnus formed a similar compound of platinum. It is possible that the greenish-yellow powder above-mentioned may bear some analogy to these, though it can hardly be of the same composition.

[†] *Kong. Vetensk. Acad Handling.* 1828.

[‡] Poggen. *Annal.* xiv. 239.

In the state in which they are obtained by the process above described, these salts seem imperfect. There must be some yet unknown process for preparing them in a better defined and more permanent form. The ammonium salt I first obtained accidentally, and in a state in which I have been enabled to preserve it, with a slight loss of colour, for several months.

I mixed a solution of sal-ammoniac with an acid solution of gold in a common wine-glass, and set it before a fire to evaporate. To hasten the evaporation I shook it up occasionally on the sides of the glass, by which they became covered with a thin film of the yellow salt. This film I found was redissolved by the acid liquid with effervescence. Concentrated in this way the solution deepened in colour to a blood red, and, on removal from the fire, deposited a great number of minute bright red cubes, mixed on cooling with minute crystals of the yellow salt, from which it was difficult by mechanical means entirely to separate them. They dissolve in water more easily than the yellow salt. A portion of these minute cubes in a close tube became yellow in a few weeks, without changing their form. Some larger crystals which I was fortunate enough to obtain on one occasion, were more permanent, insoluble in alcohol, and affecting a form derived from the cube. These crystals, as well as others of the yellow salts, Dr Brewster has kindly undertaken to examine and describe.

The salt obtained by both processes is probably the same, though, by the method last mentioned, I could not form any red sodium salt. The peculiar relations of ammonia to chemical bodies, however, are sufficient to account for this difference.

I have not analyzed any of these salts; they are obviously anhydrous, as are the red salts of the other metals described by Berzelius and Magnus. In the corresponding salt of palladium the metal is in combination with an atom of chlorine more than in the yellow salt. The phenomena attending the solution of the red gold salts, and the interchange of yellow prisms and red cubes, do not seem to argue the presence of any higher compound in them than the common *ter*-chloride of gold. The bichloride in the red salt of palladium is decomposed by warm water with evolution of chlorine; the red salts above described suffer no such decomposition. The probability is, therefore, that they differ from the yellow salts simply in being anhydrous.

V. *Yellow double chlorides of gold and the other bases.*—Except with chloride of barium I have not formed any of the other double chlorides of gold. Want of time has prevented me from introducing into this paper an intended description and analysis of the double salt of gold and lithium. In regard to the other bases I have been in a great measure anticipated by Bonsdorf, in a long and able paper inserted in the *Transactions of the Swedish Academy* for 1828, and translated into Poggendorf's *Annals*, vol. xvii. p. 115 and 247. In this paper[*] Bonsdorf describes a great number of double chlorides of mercury, gold, platinum, and palladium, under the appellation of chloro-mercuriates, chloro-goldates, &c. in illustration of a view which he entertains, that the chlorides, iodides, &c. of any particular metal, act as an acid to the chlorides, iodides, &c. of all the more electro-positive metals.[†] To render the present paper more complete, I shall subjoin what he has stated in regard to the *chloro-aurates* or double chlorides of gold. They were all formed by mixing a solution of the crystalline compound of chloride of gold and muriatic acid mentioned in the beginning of this paper, with solutions of the other metallic chlorides, and gently heating the mixture till the acid was driven off. By re-solution in water the salts easily crystallize, either by spontaneous evaporation or over sulphuric acid. They are all very soluble in water and alcohol.

1. *Chloride of Gold and Barium* forms a yellow salt in low

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[*] The results in this paper have been long kept back by the unfortunate fire at Abo in September 1827, by which, along with the university, all Bonsdorf's preparations and implements were destroyed, and his labours interrupted.

[†] The same view is entertained by Berzelius in regard to the sulphur salts, or combinations of a sulphuretted electro-negative body with the sulphuretted electro-positive metals.

rhombic prisms or tables, having an obtuse angle of 105°. It generally deliquesces and can be preserved only in a dry atmosphere. I have formed this compound, and obtained from it a *red* salt also by the method above described.

2. *Chloride of Gold and Strontium* forms a yellow salt, shooting into rhombic prisms, which are permanent in the air.

3. *Chloride of Gold and Calcium.*—This salt crystallizes in long rhombic prisms, commonly uniting sideways into a straw-like crystallization. .562 grammes of the salt, heated till the water and chlorine were driven off, left .339, from which water extracted .077 chloride of lime, leaving .262 of metallic gold. And as 262 takes 140 of chlorine, the chloride of gold weighs 402, so that, taking the deficiency for water, we have

		Calculation.	Experiment.	Chlorine.
Chloride of gold,	1 atom	73.54	71.53	24.91
Chloride of lime,	1 atom	13.45	13.70	8.68
Water,	6 atoms	13.01	14.77	
		100 *	100	

In this, as well as in the following analyses, it will be seen that Bonsdorf takes it for granted that the gold is combined with 3 atoms of chlorine, a point which it has been the object of the former part of this paper to prove.

4. *Chloride of Gold and Magnesium.*—By evaporation over sulphuric acid this compound is easily obtained in low rhombic prisms of nearly 72° and 108°. The salt, which is of a beautiful citron yellow, is permanent in winter † but deliquesces in summer. A slight heat drives off its water and melts it into a dark brown liquid, which gives off chlorine and at length dries. This salt was analyzed by exposure to a stream of hydrogen gas, and afterwards treated with dilute muriatic acid, by which the gold was obtained, evaporating the solution, and heating to redness for the magnesia, and calculating the chlorides. The result gave

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* These calculations are made from the atomic weights of Berzelius.

† The winter of Abo!

		Calculation.	Experiment.	Chlorine.
Chloride of gold,	1 atom,	= 66.10	64.50	15.91
Chloride of magnesium,	1 atom,	= 10.40	11.00	8.1
Water,	12 atoms,	= 23.50	24.50	
		100	100	

5. *Chloride of Gold and Manganese* crystallizes in yellow rhombic prisms, which deliquesce in summer, but are permanent in winter. This salt is isomorphous with the preceding.

6. *Chloride of Gold and Zinc* forms a salt in colour and general appearance like the magnesium salt, and isomorphous with it. It is permanent even in slightly moist air. Its composition is undoubtedly analogous to that of the magnesium salt.

7. *Chloride of Gold and Iron.* This salt does not exist. When the two chlorides are mixed, the iron, as is well known, takes up more chlorine, and precipitates the gold in the metallic state.

8. *Chloride of Gold and Cadmium* forms a darker yellow salt in prismatic needles, which are permanent in the air.

9. *Chloride of Gold and Cobalt* is obtained by spontaneous evaporation in long exceedingly oblique rhombic prisms. The salt is dark yellow and is unchanged in the air.

10. *Chloride of Gold and Nickel* crystallizes in low rhombic prisms, isomorphous with the zinc and magnesium salts. It deliquesces in summer, but is permanent in winter.

By solution in nitro-muriatic acid and evaporation, all these salts will probably give red anhydrous compounds.

PORTOBELLO, *7th June* 1830.

ART. XVI.—*Observations on some passages of Dr Lardner's Treatise on Mechanics.* By the Reverend W. WHEWELL, M. A., F. R. S. Professor of Mineralogy, Cambridge. In a Letter to Dr BREWSTER.

MY DEAR SIR,

I HAVE already had occasion to show, in the pages of your *Journal*, that I consider as a matter of some importance the soundness of the reasoning employed in establishing the fundamental doctrines of motion. The science of Mechanics, in its true and genuine form, is by far the most perfect specimen which we possess of the inductive philosophy:—of principles collected from extensive experiments and observations, elevated to the highest point of generality, successfully applied to the determination of phenomena most numerous and complicated. But this science will deservedly forfeit the distinction which we thus claim for it, if we become careless about the mode of proving our general principles; if we try to make them appear to be mere identical propositions; and if we interpret them in a lax and wavering manner, in order to suit the different occasions on which we may wish to apply them without the trouble of accurate deduction.

With these views I need make no apology, I trust, for offering you a few remarks on some passages in Dr Lardner's volume on Mechanics, recently published as part of his *Cabinet Cyclopædia.* Any one who takes an interest in the purity of our scientific logic, will be particularly solicitous that it should not be corrupted without notice taken, through a work which seems to have so fair a prospect of extensive circulation. And I think, that whether or not such persons may assent to my views as to the true foundations of mechanical reasoning, they will agree with me, that nothing ought to be allowed to pass without animadversion in an exposition of these doctrines, which, coming from a mathematician of acknowledged eminence, can reasonably be accused of being erroneous or unmeaning.

Dr Lardner has, in page 45, given his readers *Newton's three laws of motion* in the form in which they were stated by their author. He then proceeds to declare his opinion, that they have little or no utility, being either identical or superfluous propositions.

This assertion, I confess, I cannot but contest. Upon the most mature consideration which I have been able to give the subject, in the course of many years, I have always arrived at the conviction that these three laws, when made as distinct as is possible, are the simplest result of the analysis of the phenomena of motion, and the ultimate and general principles from which our synthesis must proceed.

If Dr Lardner, instead of taking these laws in their original form, had introduced other principles equally distinct and clear, which might supersede the use of the laws in explaining the doctrines of motion, (a proceeding which is very possible) I should not have thought any criticism necessary; but as it appears to me that his slighting notice of Newton's propositions is combined with some errors as to the nature and evidence of the principles which he has employed, and with a want of strict reasoning in his demonstrations, I shall endeavour to show, that we may yet find our advantage in adhering a little longer to this long-received code of " the three laws."

Of the " first law" Dr Lardner says, that " when *inertia* and *force* are defined, it becomes an identical proposition :" on which I have to observe, *First*, that in defining inertia he *assumes* the first law of motion ; *Secondly*, that he not only does this, but he assumes this law to be demonstrated by *a priori* proof, instead of resting on the evidence of experience.

He says " *Inertia*, or *inactivity*, signifies the total absence of power in a body to change its state of rest or motion." Now motion is here (see art. 41,) used in the sense of velocity, and to say that the body has no power to change its velocity, is to say that the velocity will not be changed except some power is exerted to produce such an effect ; it is to assert that the velocity will not undergo any change in consequence of any elementary and universal law of nature, independent of any particular circumstances of the case ; an assertion which is exactly the first law of motion.

This fallacy, which I think can hardly be denied to be one, is, it may be observed, of very frequent occurrence in Treatises on Mechanics, and the temptation to commit it seems to reside in the word *velocity*, (or *motion*, used as equivalent to velocity.) This word does, in fact, express a property or relation belonging to the space through which a body moves, and to the time which it so employs ; but this relation being once marked by a single word, the word is supposed to designate a property, not of the motion but of the body itself ; and this property is then conceived to have a permanency, like the form or size of the body, till it is altered by external agents.

The mistake, considered in this point of view, is a curious and instructive instance of the influence which words exercise upon our reasonings. It is no doubt one of the highest merits which a scientific language can possess, that it should make as simple and brief as possible the expression of true elementary and important propositions. But it was sufficiently proved to the mathematical world, by the long controversy about the two meanings of the word *force*, that the selection of the fundamental propositions, and of the definitions of terms which were most convenient, may sometimes be a matter of doubt and of choice. And it might have been supposed, that the opinions held in that case by eminent mathematicians, would have been a sufficient lesson to warn reasoners on such subjects against the mistake of supposing that they can, by choosing their definitions, make the laws of motion become necessary truths. We may, however, observe, that the general tendency to the fallacy under consideration, and the difficulty of convincing a person at first that it has been incurred, show, in a very striking manner, how completely the appropriation of a word in such a case as this, of "velocity," answers the purpose of giving to the complex idea of relation which it marks, unity of form and facility of use, and the delusion follows close on this facility. We imagine that the relation must be permanent, because it has been distinguished from all others, and is found in possession of a ready and familiar name; although, in fact, the thing designated by the name might be, so far as our means of judging are concerned, variable and fluctuating from one moment to another.

To illustrate this point a little farther, let us suppose the case to have been otherwise than it is; and let us see what changes in our language, as well as in our reasonings, would have been requisite to adapt the pretended proof above-noticed to such a case. It will, at least, involve no contradiction if we suppose that all bodies, by a universal law, tend to move more and more slowly as long as they continue in motion. Let it be supposed, that, independently of all reference to external causes, (such being of course excluded in our present speculation) a body would, in all cases, lose one thousandth (or any other given fraction) of its velocity in every second. Such a law of motion is quite consistent and possible, and would, perhaps at first sight, appear to most persons more probable than the law which really obtains. In this case, if we take two equal successive units of time, a body moving freely would describe in those times two spaces bearing a certain ratio; a ratio invariable for all bodies, spaces, and velocities, and depending only on the unit of time. And this ratio, being an element of universal occurrence and use in considering the motion of bodies, would naturally have some name fixed upon it for the sake of convenience. It might perhaps be termed "the progressive rate" of the body. It would, on the suppositions here made, be found that in all motions, except so far as they were disturbed by external causes, the *progressive rate* of bodies was the same; that whatever motion was given to a body at first, *the progressive rate*, and the consequent dependence of the space upon the time, continued unaltered. The *inertia* of a body might then naturally and properly be described as the total absence of power in a body to change its *progressive rate*; and bodies would be said to move according to the "law of inertia" when they moved with this gradually diminishing velocity.

It would, therefore, be easy for those who wrote treatises on mechanics in a universe so constituted, to make short work of the proof of this law, by enumerating inertia, SO DEFINED, among the elementary and universal properties of matter; and then by asserting, that, in virtue of this property, a body could no more alter its *progressive rate* than it could put itself in motion when at rest, or stop itself at once. All this might be said, and yet it is manifest to us, knowing such a state of things to be purely hypothetical, that no proof of such a law could be obtained but from the observation and mensuration of phenomena.

It may, moreover, be observed, that, if such a law had been true, there would have been, corresponding to each initial velocity of a body, a certain *space* which the body would describe before its motion was extinguished; and this space no circumstances, except an alteration of the initial velocity, could have increased or diminished. It would have been quite allowable to call this space the *motion* belonging to the initial velocity; and, speaking conjecturally, it appears to me probable, (on the hypothesis already made, and considering the mode in which such errors have been applied,) that it would have been so called. On this supposition, mechanical writers would have been able to assert, quite as truly and positively as they can at present, that no body has any power to increase or diminish any *quantity of motion* which it might have received.*

It seems to me curious that Dr Lardner should have been satisfied with the reasoning which he has employed in the proof of this law, or property as he terms it, of inertia, since he is thus led to use expressions which are at variance with his own observations on the proper phraseology for this subject. In p. 7 he makes an objection (as appears to me not well founded,) to the use of the term *force* for the *cause* of motion; and he adds, in the way of reason for his objection, that, "when causes are referred to, it is implied that effects of the same class arise from the agency of the same cause. However probable this assumption may be," he says, "it is altogether unnecessary." But at page 28, when he is proving the property of inertia in his sense of uniform velocity, he says: "the same power which would cause a body moving at ten miles an hour to increase its rate to eleven miles, would also cause the same body at rest to commence moving at the rate of one mile an hour." Here it is assumed, that the *same cause* which augments the velocity of a body already in motion, must necessarily reside and operate in the same body at rest. Indeed, there is in this case a good deal more taken for granted. It is implied that this cause would not only operate in the two cases, but, moreover, that it would operate according to certain rules which belong to the *second* law of motion; adding exactly as much velocity to a body already in motion as it would have communicated to the same body at rest. However true this supposition may be, manifestly it ought neither to be asserted without proof when it is introduced, nor introduced at all in proving a proposition more elementary than itself; and one which is necessarily supposed when the rule concerning the addition of velocity is established.

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* It is easy to obtain the formulæ for the motion of bodies which would be true in the case supposed. If $1 + n : 1$ be the ratio of the velocity in two successive seconds, c the initial velocity, v that after t seconds, $v = c(1 + n)^{-t}$. Also the whole space described with the initial velocity a will be $c\,\frac{1+n}{n}$. If $n = \frac{1}{1000}$, the ratio of the velocities in successive seconds will be 1001 : 1000, and the whole space described, $1001.c$.

I am afraid many of your readers will consider these discussions as trifling and unprofitable subtleties; and will think that it cannot be a matter of any consequence in which way we prove that which all allow to be true. In excuse of my occupying your pages with remarks on these subjects, I might plead, that sound thinking and accurate expression have usually been considered as essential to the respectability of science; and, that if its more general diffusion is to be accompanied by a contempt of these qualities, the advantages of the spread of knowledge will be grievously diminished. But, if such a doctrine be thought to be of too severe a cast for modern times, at any rate we may venture to say, that, if it is not worth while being right in such points, it cannot be necessary to waste words on them at all; and that those who deliver a decided and professional opinion upon these reasonings, impose upon themselves the obligation of that patience and accuracy of thought which such investigations require. Above all it is unreasonable to treat slightingly the abstruser speculations belonging to the fundamental points of mechanical philosophy, and at the same time to carry these speculations so far, and to conduct them so erroneously, as to lose sight of the dependence of our principles upon experience. It is doing great injustice to this magnificent department of human knowledge, to exhibit its doctrines as a set of consequences flowing merely from the relations of certain abstract terms and arbitrary definitions, instead of making them, as they ought to be made, a truly inductive science, concerned with, and collected from the observation of physical facts. The collection, the analysis, and the generalization of these facts, occupied several generations of acute and laborious intellects; and that the principles now appear almost self-evident, shows how completely the task was executed, and how well the expressions belonging to the science answer the purposes contemplated in their selection; but we shall make a strangely perverse use of these advantages, if we begin to imagine that the laws of motion are constituted or upheld by the words which are convenient for their exposition.

I am persuaded that Dr Lardner will not be disposed to take offence at these observations, which have for their object to place in the true point of view the fundamental principles of a science which he has successfully cultivated. It will be clear to all your readers, that what has been said implies no blemish in that part of the treatise which is employed in the application of these principles.—I am, my dear Sir, yours, &c.

TRINITY COLLEGE, CAMBRIDGE, W. WHEWELL.
May 10, 1830.

ART. XVII.—*On the Improvement of the Microscope.* By H. CODDINGTON, M. A. F. R. S. Fellow of Trinity College, and of the Cambridge Philosophical Society.*

AMONG the numerous excellent suggestions which Dr Brewster has from time to time thrown out to those engaged in the theory or the practice of Optics, there is one which appears to have been most unworthily, and most unaccountably, neglected. It is that of substituting a *sphere* for a *lens* in the construction of a microscope. This is the more surprising, as many persons of great eminence have, of late years, turned their attention to the improvement of this instrument, in which pursuit they have spared neither time, labour, nor expence. The only reason which I can give for this is, that as, until the investigations of Professor Airy, which are contained in the present volume of the *Transactions* of this Society, nobody, with the exceptions of Dr Young and Dr Wollaston, ever dared to approach the thorny subject of the oblique refraction of a pencil of rays by a lens; almost all other persons have been satisfied with endeavouring to show, as distinctly as possible, one individual point of an object, trusting that the rest would follow of itself; or giving up, as hopeless, the idea of producing a good and large field of view.

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* Slightly abridged from the *Transactions of the Cambridge Philosophical Society.*

To those who have studied the construction of the compound microscope, an analogy presents itself, very naturally, between that instrument and the telescope. In each there is an image formed, which is seen through one or more lenses, constituting what is technically termed in the former case the *body*, in the latter the *eye-piece*.

The progress of these instruments has been curiously similar in some respects. The first step of any consequence in the case of the telescope, was Huyghens's eye-piece, which, besides the merit supposed by its author, of diminishing the errors arising from aberration, had one, much more important, which he did not contemplate, the correction of the coloured fringes, seen about every part of the image, except that precisely in the centre. Ramsden then succeeded in making an eye-piece, which gives a flat field of view, when that point is particularly important, and finally, the instrument has been made perfect, by substituting for the simple object glass, an achromatic and aplanatic combination of lenses. In the compound microscope, the first point, (the correction of the coloured fringes,) has been completely attained; on the second, much labour has been bestowed by practical opticians, but with little success; the third has lately occupied some of the most distinguished theorists and artisans, who have been eminently successful, but the difficulty and expense necessarily attending their processes, are so great, that but few persons can derive any benefit from their exertions.*

In making a comparison between the telescope and microscope, it must be observed, that some difficulties, and sources of error, which in the former are so small as to have been overlooked, are in the latter of the greatest and most palpable importance. The image produced by the object glass of a telescope is usually considered as perfectly plane, and equally distinct in all its parts, and this supposition is quite sufficiently accurate, because although the image given by a lens with centrical pencils, is on the whole very much curved and very indistinct, so small a part of it is employed in this case, and that only the most perfect, that the defects are usually quite insensible in practice.

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* Mr Tulley has just finished an achromatic microscope ordered for Lord Ashley, about six months ago. This instrument, which I have seen, is a masterpiece of art, but I believe that the above eminent optician has been obliged to make the object-glass with his own hands, and the price is far beyond the reach of most naturalists.

I have shown, (*Treatise on the Reflection and Refraction of Light*, Art. 145.) after Dr Young and Professor Airy, that if we represent by

λ, the aperture of the object glass,

z, the distance of a point of the image from the axis,

f, the focal length of the lens,

k, the distance of the image from the lens,

the indistinctness is proportional to the diameter of the least space over which a pencil is diffused, the value of which is $\frac{\lambda z^2}{2kf}$.

Now in a telescope, z being nearly equal to the semi-aperture of the field-glass, is very much less than f, to which k is equal, and as a high magnifying power is produced by means of a powerful eye-piece, applied to an object glass which is never changed, and as the apertures of the lenses used for eye-pieces, of the same kind, are usually proportional to their focal lengths, the higher the magnifying power, the less is the fraction $\frac{z^2}{kf}$ or $\frac{z^2}{f^2}$. For instance, in a five foot telescope, it is seldom, if ever, greater than $\frac{1}{14400}$, and often very much less, so that the value of the quantity $\frac{\lambda z^2}{2f^2}$ is about $\frac{1}{7200}$.

In a microscope, on the other hand, f is a very small quantity, though k is not so, and the magnifying power is raised by applying an object-glass of shorter focus to the same body. The following values are, I believe, such as might fairly occur: $\lambda = \frac{1}{20}$, $z = \frac{1}{2}$, $f = \frac{1}{4}$, $k = 3$. These give $\frac{\lambda z^2}{2kf} = \frac{1}{60}$, which as, with different object-glasses, z and k are constant, and λ usually proportional to f, may be considered as its general value.

Again, in a telescope, the portion of the image used is sensibly flat, though the radius of curvature of every such image, is about $\frac{5}{8}$ths of the focal length of the object-glass: but in the microscope it is evidently far otherwise, so that were the whole image distinct, it would still be impossible to have any great extent of it distinctly visible at once; and this objection applies in full force to the most perfect achromatic object-glass. Now with a sphere, properly cut away at the centre so as to reduce the aberration, and dispersion, to insensible quantities, which may be done most completely and most easily, as I have found in practice, the whole image is perfectly distinct, whatever extent of it be taken, and the radius of curvature of it is no less than the focal length, so that the one difficulty is entirely removed, and the other at least diminished to one-half.

Besides all this, another advantage appears in practice to attend this construction, which I did not anticipate, and for which I cannot now at all account. I have stated that when a pencil of rays is admitted into the eye, which, having passed without deviation through a lens, is bent by the eye, the vision is never free from the coloured fringes produced by excentrical dispersion. Now with the sphere I certainly do not perceive this defect, and I therefore conceive that if it were possible to make the spherical glass on a very minute scale, it would be the most perfect simple microscope, except perhaps Dr Wollaston's doublet, than which I can hardly imagine anything more excellent as far as its use extends, its only defects being the very small field of view, and the impracticability of applying it, except to transparent objects, seen by transmitted light.

Now the sphere has this advantage, that whereas it makes a very good simple microscope, it is more peculiarly fitted for the object-glass of a compound instrument, since it gives a perfectly distinct image of any required extent, and that, when combined with a proper eye-piece, it may without difficulty be employed for opaque objects. I have, therefore endeavoured so to combine it, and this has been my principal difficulty; for the systems of lenses which I have found employed for this purpose, are so improperly constructed, that I have been forced to have one made from original calculations, and get tools constructed on purpose, which has necessarily been attended with some delay.

The principle which I have adopted, after one or two previous trials, may be explained as follows.

One great cause of the excellency of Huyghen's eye-piece, is the condition which he himself designed to fulfil, namely, that the bending of the pencil is equally divided between the two lenses. Now this may be done for a microscope, thus:

Let O (Fig. 2, Plate I.) be the centre of the object-glass,
F the place of the field-glass,
E ———— eye-glass.
Let $OF = 2$ inches, (for example)
$FE = 1$ inch.
And let the focal length of the field-glass be 1 inch,
———————— eye glass — ½ inch

These values satisfy the conditions of achromatism, and it will easily be seen, that if Y be the place where the pencil tends to cross the axis after refraction at the field glass, and z that where it actually crosses after emerging from the eye-glass, the angle of flexure, at each lens, is double of the original inclination of the pencil to the axis.

This simple system is, however, not applicable, as it is impossible to satisfy the condition necessary for perfect distinctness, much less that for destroying, as far as possible, the convexity of the field. These may, however, be very readily satisfied by employing two lenses of equal power, in each place, instead of one. The most proper forms of the lenses are those shown in Fig. 3, the field-glasses and the second eye-glass being of the meniscus form, and the first eye-glass equi-convex. I have found no sensible error arise from the substitution of plano-convex lenses for the meniscus glasses, which are difficult and expensive to form. Theory indicated a further flattening of the field, to be made by separating the eye-glasses a little, which requires the distance of the first eye-glass from the field-glasses, to be diminished by about half as much; I cannot say, however, that I perceive any improvement arising from this alteration, in practice, and as the field is quite flat enough with the eye-glasses in contact, and any further diminution of the apparent convexity, can be gained only by a sacrifice of distinctness, I cannot on the whole recommend it. I have not, however, yet had the instrument in a sufficiently perfect state of adjustment, in other respects, to be able to give a decided opinion on this point. This system, as it will easily be seen, gives a magnifying power of 3 to the eye-piece, so as to multiply, by that number, the power of the object-glass. It would be easy, if necessary, to produce a higher magnifying power, by employing lenses of shorter focal lengths, regard being had, in each case, to the proper condition of achromatism. Thus several different eye-pieces might be inserted at pleasure into one tube, in the same manner as it is usual to vary the magnifying power of a telescope. I have not yet tried the effect of this, but I suppose it may be necessary in applying the microscope to opaque objects, as the difficulty of illuminating them almost precludes the use of a powerful object-glass.

I do not pretend to give this as a perfect instrument—much less as one that will answer all purposes; but having tried it in a very rough state, and with a moderate magnifying power, on various delicate test objects, all of which it shows very satisfactorily, not excepting the striæ on the scales of the Podura, which Mr Pritchard, the inventor of the diamond and sapphire lenses, says are only just discernible with the most perfect instruments, I see no reason to doubt that, when carefully executed, it will be found very effective, and that the naturalist may be furnished, at an expence not exceeding five or six guineas, with a microscope which will perform nearly all that can be expected from that instrument.

Fig. 4. represents the microscope, as I have directed it to be made by Mr Cary.

TRINITY COLLEGE, H. CODDINGTON.
April 23, 1830.

ART. XVIII.—*On the law of the partial polarization of Light by reflexion.* By DAVID BREWSTER, LL. D. F.R.S. L. & E.[*]

IN the year 1815 I communicated to the Royal Society a series of experiments on the polarization of light by successive reflexions, which contain the germ of the investigations, the results of which I now propose to explain.

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[*] From the *Phil. Trans.* 1830. Part I. p. 69—84.

From these experiments it appeared that a given pencil of light could be wholly polarized at any angle of incidence, provided it underwent a sufficient number of reflexions either at angles wholly above or wholly below the maximum polarizing angle, or at angles partly above and partly below that angle; and it was scarcely possible to resist the conclusion that the light not polarized by the first reflexion had suffered a physical change at each action of the reflecting force which brought it nearer and nearer to the state of complete polarization. This opinion, however, which I have always regarded as demonstrable, appeared in a different light to others. Guided probably by an experimental result, apparently though not really hostile to it, Dr Young and MM. Biot, Arago, and Fresnel have adhered to the original opinion of Malus, that the reflected and refracted pencils consist partly of light wholly polarized, and partly of light in its natural state; and more recently Mr Herschel has given the weight of his opinion to the same view of the subject.

Under these circumstances, I have often returned to the investigation with renewed zeal; but though the frequent repetition of my experiments has more and more convinced me of the truth of the conclusions which I drew from them, yet I have not till lately been able to place the subject in a satisfactory aspect, and to connect it with general laws, which give a mathematical form to this fundamental branch of the science of polarization.

If we consider a pencil of natural light as divided into two pencils polarized in rectangular planes by the action of a doubly refracting crystal, and conceive the light of these two pencils to return back through the crystal, it will obviously emerge in the state of natural light. When we examine the pencil thus recomposed, or when we examine a pencil consisting of two oppositely polarized pencils superposed, we shall find that they comport themselves under every analysis exactly like common light; so that we are entitled to assume such a pencil as the representative of natural light, and to consider every thing that can be established respecting the one, as true respecting the other.

In applying this principle to the analysis of the phenomena produced by reflexion, I placed the planes of polarization of the compound beam in the plane of reflexion; but though this led to some interesting conclusions, it did not develope any general law. I then conceived the idea of making the plane of reflexion bisect the right angle formed by the planes of polarization; and in this way I observed a series of symmetrical effects at different angles of incidence, which threw a broad light over the whole subject.

In order to explain these results, let A, B, Plate I. (Fig. 5,) represent the two pencils of oppositely polarized light as separated by double refraction; let *a b*, *c d* be the directions of their planes of polarization, forming a right angle *a e c*, and let the plane of reflexion M N, of a surface of plate glass, bisect the angle *a e c*, so that the planes *a b*, *c d* form angles of +45° and —45° with the plane M N. Let a rhomb of calcareous spar have its principal section now placed in the plane of reflection.

At an incidence of 90°, reckoned from the perpendicular, the reflected images of A and B suffer no change, the angle *a e c* is still a right angle, and the four pencils formed by the calcareous spar are all of equal intensity. As the incidence, however, diminishes, the angle *a e c* diminishes also, and the ordinary and extraordinary images of A and B differ in intensity. At an incidence of 80° for example, the angle *a e c* is reduced from 90° to 66°; at 70° it has been reduced to 40°, and at 56° 45', the maximum polarizing angle, it has been reduced to 0°; that is, the planes of polarization *a b*, *c d* are now parallel. Below the polarizing angle, at 50°, the axes are again inclined to each other, and form an angle of 22°. At 40° they form an angle of 50°, and at 0°, or a perpendicular incidence, they are again brought back to their primitive inclination of 90°. Taking M N to represent the quadrant of incidence from 90° at M, to 0° at N, the curves, 90°, 0°, show the progressive change which takes place in the planes of polarization, the plane of polarization being a tangent to the curve at the incidence which corresponds to any particular point of it.

When we employ a surface of diamond in place of glass, the inclination of the axes *a b*, *c d* is reduced to 46° at an incidence of 80°, to 8° at an incidence of 70°, and at 67° 43′ the axes become parallel.

Such being the action of the reflecting forces upon A and B taken separately, let us now consider them as superposed and forming natural light. At 90° and 0° of incidence, the reflecting force produces no change in the inclination of their axes or planes of polarization; but at 56° 45′ in the case of glass, and 67° 43′ in the case of diamond, the axes of all the particles are brought into a state of parallelism with the plane of reflection; and consequently when the image which they form is viewed by the rhomb of calcareous spar, they will all pass into the ordinary image, and thus prove that they are wholly polarized in the plane of reflection.

All this is entirely conformable to what has been long known: but we now see that the total polarization of the reflected pencil at an angle whose tangent is the index of refraction, is effected by turning round the planes of polarization of one-half of the light from right to left, and of the other half from left to right, each through an angle of 45°. Let us now see what takes place at those angles where the pencil is only partially polarized At 80° for example, the angle of the planes *a b*, *c d* is 66°, that is, each plane of polarization has been turned round in opposite directions from an inclination of 45° to one of 33° with the plane of reflection. The light has therefore suffered a physical change of a very marked kind, constituting now neither natural nor polarized light. It is not natural light, because its planes of polarization are not rectangular; it is not polarized light, because they are not parallel. It is a pencil of light having the physical character of one-half of its rays being polarized at an angle of 66° to the other half. It will now be asked, how a pencil thus characterized can exhibit the properties of a partially polarized pencil, that is, of a pencil part of whose light is polarized in the plane of reflexion, while the rest retains its condition of natural light. This will be understood by replacing the analyzing rhomb with its principal section in the plane of reflexion, and viewing through it the images A and B at 80° of incidence. As the axis of A is inclined 33° to M N or the section of the rhomb, the ordinary image of it will be much brighter than the extraordinary image the intensity of each being in the ratio of $\cos^2 \varphi$ to $\sin^2 \varphi$, φ being the angle of inclination, or 33° in the present case. In like manner the ordinary image of B will be in the same ratio brighter than its extraordinary image, that is, by considering A and B in a state of superposition, the extraordinary image of a pencil of light reflected at 80° will be fainter than the ordinary image in the ratio of $\sin^2 33°$ to $\cos^2 33°$. But this inequality in the intensity of the two pencils is precisely what would be produced by a compound pencil, part of which is polarized in the plane of reflexion, and part of which is common light. When Malus, therefore, and his successors analyzed the pencil reflected at 80°, they could not do otherwise than conclude that it was partially polarized, consisting partly of light polarized in the plane of reflexion, and partly of natural light. The action of successive reflexions, however, afforded a more precise means of analysis, in so far as it proved that the portion of what was deemed natural light had in reality suffered a physical change, which approximated it to the state of polarized light; and we now see that the portion of what was called polarized light was only what may be called apparently polarized; for though it disappears, like polarized light, from the extraordinary image of the analysing prism, yet there is not a single particle of it polarized in the plane of reflexion.

These results must be admitted to possess considerable interest in themselves; but, as we shall proceed to show, they lead to conclusions of general importance. The quantity of light which disappears from the extraordinary image, is obviously the quantity of light which is really or apparently polarized at the given angle of incidence; and if we admit the truth of the law of repartition discovered by Malus, and represented by $P_{oo} = P_o \cos^2 \varphi$, and $P_{oe} = P_o \sin^2 \varphi$, and if we can determine φ for substances of every refractive power, and for all angles of incidence, we may consider as established the mathematical law which determines the intensity of the polarized pencil, whatever be the nature of the body which reflects it,—whatever be the angle at which it is incident,—whatever be the number of reflexions which it suffers, and whether these reflexions are all made from one substance, or partly from one substance and partly from another.

The first step in this investigation is to determine the law according to which a reflecting surface changes the plane of polarization of a polarized ray. This subject was first examined by Malus, but not with that success which attended most of his labours. Before I was acquainted with what had been done by M. Fresnel, or with the experiments of M. Arago on glass and water, I had made a number of very careful experiments on the same subject, and had represented them by formulæ founded on the law of the tangents. These formulæ, however, I found to be defective; and I am persuaded, from a very extensive series of experiments, that the formulæ of Fresnel are accurate expressions of the phenomena under every variation of incidence and refractive power. If i is the angle of incidence, i' the angle of refraction, x the primitive inclination of the plane of the polarized ray to the plane of reflexion, and φ the inclination to which that plane is brought by reflexion, then, according to Fresnel, we have

$$\text{Tan}\ \varphi = \tan x \frac{\cos(i + i')}{\cos(i + i')}$$

When x is 45°, as in the preceding observations, then $\tan x = 1$, and we have

$$\text{Tan}\ \varphi = \frac{\cos(i + i')}{\cos(i - i')}.$$

In these formulæ, which are founded on the law of the tangents, $i + i'$ is the supplement of the angle which the reflected ray forms with the refracted ray; while $i - i'$ is the angle which the incident ray forms with the refracted ray, or the deviation produced by refraction.

These formulæ have been verified by M. Arago at ten angles of incidence upon glass, and four upon water; but his experiments were made only in the case where x is 45°, and where tan x disappears from the formula. As my experiments embrace a wider range of substances, and also the general case where x varies from 0° to 90°, I consider them as a necessary basis for a law of such extensive application.

The first series of experiments which I made was upon plate glass, in which the maximum polarizing angle was nearly 56°: hence I assume the index of refraction to be 1.4826. The following were the results:

Plate Glass.

Angle of Incidence.	Angle of Refraction.	Inclination of Plane of Polarization to Plane of Reflection. Observed.	Computed.	Difference.
90°	0° 0°	45° 0′	45° 0′	0° 0′
88	42 23	43 4	42 49	+0 35
86	42 17	40 43	40 36	+0 7
84	42 8	38 47	38 22	+0 25
80	41 37	33 13	33 46	—0 33
75	40 40	28 45	27 41	+1 4
70	39 20	22 6	21 3	+1 3
65	37 41	14 40	13 53	+0 47
60	35 45	6 10	6 16	—0 6
56	34 0	0 0	0 0	0 0
50	31 22	9 0	9 0	0 0
45	28 29	16 55	16 31	+0 24
40	25 42	22 37	23 1	—0 24
30	19 43	32 25	33 19	—0 54
20	13 20	39 0	40 4	—1 4
10	6 44	44 0	43 49	+0 11

These results, obtained in every part of the quadrant, completely establish the accuracy of the formula. The differences are all within the limits of the errors of observation, and amount, at an average, to 32½′ on each observation.

In order to establish the accuracy of the formula for different degrees of refractive power, I made the following experiments on diamond, in which the index of refraction was 2.440.

Diamond.

Angle of Incidence.	Angle of Refraction.	Inclination of Plane of Polarization to Plane of Reflexion. Observed.	Calculated.	Difference.
90° 0′	24° 12′	45° 0′	45° 0′	0° 0′
85 0	24 6	34 30	33 56	+0 34
80 0	23 48	24 0	23 12	+0 48
75 0	23 19	14 30	13 8	+1 22
70 0	22 39	4 30	3 54	+0 36
67 43	22 17	0 0	0 0	0 0
60 0	20 47	12 30	11 41	+0 49
50 0	18 18	24 0	23 30	+0 30

These differences, which at an average amount to $46\frac{1}{2}'$, are also within the limits of the errors of observation.

In all these experiments the value of x was 45°; but in order to determine the law of variation for φ, when x varies from 0° to 90°, I took a crystal of quartz with a fine natural surface parallel to its axis; and I found that at an angle of incidence of 75°, and when x was 45°, the inclination of the plane of polarization to the plane of reflexion was 26°20′. I then varied x, and obtained the following results:

Values of x.	Inclination of Plane of Polarization.		Difference.
	φ Observed.	φ Calculated.	
0°	0° 0′	0° 0′	0° 0′
10	4 54	4 29	+0 25
20	10 0	10 16	—0 16
30	15 50	16 2	—0 12
35	20 0	19 12	+0 48
40	23 30	22 40	+0 50
45	26 20	26 27	—0 7
50	30 0	30 40	—0 40
55	35 30	35 23	+0 7
60	40 0	40 45	—0 45
70	53 0	53 49	—0 49
80	70 0	70 29	—0 29
90	90 0	90 0	0 0

In these experiments the average error does not exceed half a degree. The third column is computed by the formula $\tan\varphi = (\tan 26°\,27')\tan x$.

From these experiments it appears that the formula expresses with great accuracy all the changes in the planes of polarization which are produced by a single reflexion, and we may therefore apply it in our future investigations.

Let us now suppose that a beam of common light composed of two portions A, B, (Plate I, Fig. 6.) polarized + 45° and — 45° to the plane of reflexion, is incident on a plate of glass at such an angle that the reflected pencil composed of C and D has its planes of polarization inclined at an angle φ to the plane M N. When a rhomb of calcareous spar has its principal section in the plane M N, it will divide the image C into an extraordinary pencil E and an ordinary one F; and the same will take place with D, G being its extraordinary and H its ordinary image. If we represent the whole of the reflected pencil or C + D by 1, then C $= \frac{1}{2}$, D $= \frac{1}{2}$, E + F $= 1$, and G + H $= 1$. But since the planes of polarization of C and D are each inclined φ degrees to the principal section of the rhomb, the intensity of the light of the doubly refracted pencils will be as $\sin^2\varphi : \cos^2\varphi$; that is, the intensity of E will be $\frac{1}{2}\sin^2\varphi$, and that of F, $\frac{1}{2}\cos^2\varphi$. Hence it follows that the difference of these pencils, or $\frac{1}{2}\sin^2\varphi - \frac{1}{2}\cos^2\varphi$, will express the quantity of light which has passed from the extraordinary image E into the ordinary one F, that is, the quantity of light apparently polarized in the plane of reflexion MN. But as the same is true of the pencil D, we have $2\left(\frac{1}{2}\sin^2\varphi - \frac{1}{2}\cos^2\varphi\right)$ or $\sin^2\varphi - \cos^2\varphi$ for the whole of the polarized light in a pencil of common light C + D. Hence, since $\sin^2\varphi + \cos^2\varphi = 1$ and $\cos^2\varphi = 1 - \sin^2\varphi$, we have for the whole quantity of polarized light

$$Q = 1 - 2\sin^2\varphi.$$

But $$\operatorname{Tan}\varphi = \tan x\,\frac{\cos(i+i')}{\cos(i-i')}$$

And as $$\operatorname{Tan}^2\varphi = \frac{\sin^2\varphi}{\cos^2\varphi},\ \text{and}\ \sin^2\varphi + \cos^2\varphi = 1,$$

we have the quotient and the sum of the quantities $\sin^2\varphi$ and $\cos^2\varphi$, by which we obtain

$$\operatorname{Sin}^2\varphi = \frac{1}{\dfrac{1}{\left(\tan x\,\dfrac{\cos(i+i')}{\cos(i-i')}\right)^2} + 1} = \frac{\left(\tan x\,\dfrac{\cos(i+i')}{\cos(i-i')}\right)^2}{1 + \left(\tan x\,\dfrac{\cos(i+i')}{\cos(i-i')}\right)^2}$$

That is $$Q = 1 - 2\,\frac{\left(\tan x\,\dfrac{\cos(i+i')}{\cos(i-i')}\right)^2}{1 + \left(\tan x\,\dfrac{\cos(i+i')}{\cos(i-i')}\right)^2}$$

As the quantity of reflected light is here supposed to be 1, we may obtain an expression of Q in terms of the incident light by adopting the formula of Fresnel for the intensity of a reflected ray. Thus

$$Q = \frac{1}{2}\left(\frac{\sin^2(i-i')}{\sin^2(i+i')} + \frac{\tan^2(i-i')}{\tan^2(i+i')}\right)\left(1 - 2\,\frac{\left(\dfrac{\cos(i+i')}{\cos(i-i')}\right)^2}{1 + \left(\dfrac{\cos(i+i')}{\cos(i-i')}\right)^2}\right)$$

As $\tan x = 1$ in common light, it is omitted in the preceding formula.

This formula may be adapted to partially polarized rays, that is, to light reflected at any angle different from the angle of maximum polarization, provided we can obtain an expression for the quantity of reflected light.

M. Fresnel's general formula has been adapted to this species of rays, by considering them as consisting of a quantity a of light completely polarized in a plane making the angle x with that of incidence, and of another quantity $1 - a$ in the state of natural light. Upon this principle it becomes

$$I = \frac{\sin^2(i - i')}{\sin^2(i + i')} \cdot \frac{1 + a\cos^2 x}{2} + \frac{\tan^2(i - i')}{\tan^2(i - i')} \cdot \frac{1 - a\cos^2 x}{2}.$$

But as we have proved that partially polarized rays are rays whose planes of polarization form an angle of $2x$ with one another as already explained, x being greater or less than 45°, we obtain a simpler expression for the intensity of the reflected pencil, viz. the very same as that for polarized light.

$$I = \frac{\sin^2(i - i')}{\sin^2(i + i')}\cos^2 x + \frac{\tan^2(i - i')}{\tan^2(i + i')}\sin^2 x$$

Hence we have

$$Q = \left(\frac{\sin^2(i - i')}{\sin^2(i + i')}\cos^2 x + \frac{\tan^2(i - i')}{\tan^2(i + i')}\sin^2 x\right.$$

$$\left(1 - 2\frac{\left(\tan x\,\frac{\cos(i + i')}{\cos(i - i')}\right)^2}{1 + \left(\tan x\,\frac{\cos(i + i')}{\cos(i - i')}\right)^2}\right)$$

This formula is equally applicable to a single pencil of polarized light of the same intensity as the pencil of partially polarized light. In all these cases it expresses the quantity of light really or apparently polarized in the plane of reflexion.

In order to show the quantity of light polarized at different angles of incidence, I have computed the following table for common light, and suited to glass in which $m = 1.525$.

PLATE GLASS.

Angle of Incidence. *i*.		Angle of Refraction. *i′*		Inclination of Plane of Polarization to Plane of Reflexion, φ.		Quantity of Light reflected out of 1000 Rays.	Quantity of Polarized Light Q.	Ratio of Polarized to Reflected Light.
°	′	°	′	°	′			
0	0	0	0	45	0	43.28	0	0
10	0	6	32	43	51	43.39	1.74	0.04000
20	0	12	58	40	13	43.41	7.22	0.16618
25	0	16	5	37	21	43.64	11 6	0.26388
30	0	19	8½	33	40	44.78	17.25	0.3853
35	0	22	6	29	8	46.33	24.37	0.5260
40	0	24	56	23	41	49.10	33.25	0.6773
45	0	27	37½	17	22½	53.66	44.09	0.82167
50	0	30	9	10	18	61.36	57.36	0.9360
56	45	33	15	0	0½	79.5	79.5	1.000
60	0	34	36	5	4	93.81	91.6	0.9628
65	0	36	28	12	45	124.86	112.7	0.90258
70	0	38	2	18	32	162.67	129.80	0.79794
75	0	39	18	26	52	257.26	152.34	0.59154
78	0	39	54	30	44	329.95	157.67	0.47786
79	0	40	4	31	59	359.27	157.69	0.43892
80	0	40	13	33	13	391.7	156.6	0.40000
82	44	40	35	36	22	499.44	145.4	0.29112
84	0	40	42	38	2	560.32	134.93	0 2408
85	0	40	47	39	12	616.28	123.75	0.2008
86	0	40	51	40	22.7	676.26	108.67	0.16068
87	0	40	54	41	32	744.11	89.83	0.12072
88	0	40	57½	42	42	819.9	65.9	0.0804
89	0	40	58	43	51	904.81	36.32	0.04014
90	0	40	58	45	0	1000.0	0	0.0000

As the preceding formula is deduced from principles which have been either established by experiment or confirmed by it, it may be expected to harmonize with the results of observation. At all the limits where the pencil is either wholly polarized or not polarized at all, it of course corresponds with experiment: but though in so far as I know there have been no absolute measures taken of the quantity of polarized light at different incidences, yet we are fortunately in possession of a set of experiments by M. Arago, who has ascertained the angles above and below the polarizing angle at which glass and water polarize the same proportion of light. In no case has he measured the absolute quantity of the polarized rays; but the comparison of the values of Q at those angles at which he found them in equal proportions, will afford a test of the accuracy of the formula. This comparison is shown in the following table, in which col. 1, contains the angles at which the reflecting surface polarizes equal proportions of light; col. 2, the values of φ or the inclination of the planes of polarization; and col. 3, the intensities of the polarized light computed from the formula.

	Angles of. Incidence i.	Inclination of planes of polarization to M N, or φ.	Proportion of polarized light or Q.
Glass: No. 1.	82° 33′	37° 33′	.2572
	24 18	37 21	.2637
No. 2.	82 5	36 47	.2828
	26 6	36 0	.3090
No. 3.	78 20	32 38	.4186
	29 42	33 1	.4064
Water: No. 4.	86 31	41 54	.1080
	16 12	41 27	.1236

The agreement of the formula with experiments made with as great accuracy as the subject will admit must be allowed to be very satisfactory. The differences are within the limits of the errors of observation, as appears from the following table:

		Deviations from Experiment.	Part of the whole light.
Glass:	No. 1.	0.0065	$\frac{1}{154}$
	No. 2.	0.0262	$\frac{1}{38}$
	No. 3.	0.0122	$\frac{1}{82}$
Water:	No. 4.	0.0156	$\frac{1}{64}$

M. Arago has concluded, from the experiments above stated, that equal proportions of light are polarized at equal angular distances from the angle of complete polarization. Thus in glass No. 1. the mean of 82° 48′ and 24° 18′ is 53° 33′, which does not differ widely from the maximum polarizing angle, or 55°, which M. Arago considers as the maximum polarizing angle of the glass.* In order to compare this principle with the formula, I found that in water No. 4. the angle which polarizes almost exactly the same proportion of light as the angle of 86° 31′, is 15° 10′, the value of φ being 41° 54′ at both these angles; but the mean of these is 50° 50′ in place of 53° 11′; so that the rule of M. Arago cannot be regarded as correct, and cannot therefore be employed, as he proposes, to determine the angle of complete polarization. *

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* Hence we have assumed $m = 1.428$, the tangent of 55°, in the preceding calculations.

The application of the law of intensity to the phenomena of the polarization of light by successive reflexions, forms a most interesting subject of research. No person, so far as I know, has made a single experiment upon this point, and those which I have recorded in the *Philosophical Transactions* for 1815, have, I believe, never been repeated. All my fellow labourers, indeed, have overlooked them as insignificant, and have even pronounced the results which flow from them to be chimerical and unfounded. Those immutable truths, however, which rest on experiment, must ultimately have their triumph; and it is with no slight satisfaction, that, after fifteen years of unremitted labour, I am enabled not only to demonstrate the correctness of my former experiments, but to present them as the necessary and calculable results of a general law.

When a pencil of common light has been reflected from a transparent surface, at an angle of 61° 3′ for example, it has experienced such a physical change, that its planes of polarization form an angle of 6° 45′ each with the plane of reflexion. When it is incident on another similar surface at the same angle, it is no longer common light in which $x = 45°$, but it is partially polarized light in which $x = 6° \; 45'$. In computing, therefore, the effect of the second reflexion, we must take the general formula $\tan \varphi = \tan x \left(\frac{\cos(i + i')}{\cos(i - i')}\right)$; but, as the value of x is always in the same ratio to the value of φ, however great be the number of reflexions, we have $\tan \theta = \tan^n \varphi$ for the inclination θ to the plane of reflexion produced by any number of reflexions n, φ being the inclination for one reflexion. Hence when θ is given by observation, we have $\tan \varphi = \sqrt[n]{\theta}$. The formula for any number n of reflexions is therefore $\tan \varphi = \left(\frac{\cos (i + i')}{\cos (i + i)}\right)^n$. It is evident that θ never can become equal to 0°; that is, that the pencil cannot be so completely polarized by any number of reflexions at angles different from the polarizing angle, as it is by a single reflexion at the polarizing angle; but we shall see that the polarization is sensibly complete in consequence of the near approximation of θ to 0°.

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* It is obvious that the rule can only be true when $m = 1.000$; so that its error increases with the refractive power.

I found, for example, that light was polarized by two reflections from glass at an angle of 61° 3′, and 60° 28′ by another observation. Now in these cases we have

	θ after 1st Reflexion.	θ after 2d Reflexion.	Quantity of Unpolarized Light.
Two reflexions at 61° 3′	6° 45′	0° 47′	0.00037
60 28	5 38	0 38	0.00018

The quantity of unpolarized light is here so small as to be quite inappreciable with ordinary lights.

In like manner I found that light was completely polarized by five reflexions at 70°. Hence by the formula we have

	Values of θ.	Unpolarized Light.
1 reflexion at 70°	20° 0′	0.23392
2 - - - - -	7 32	0.03432
3 - - - -	2 45	0.00460
4 - - - -	1 0	0.00060
5 - - - - -	0 22	0.00008

The quantity of unpolarized light is here also unappreciable after the fifth reflexion.

In another experiment I found that light was wholly polarized by the separating surface of glass and water at the following angles:

	Values of θ.	Unpolarized Light.
By 2 reflexions at 44° 51′	0° 56′	0.0005
By 3 - - 42 27	0 26	0.0001

In all these cases the successive reflexions were made at the same angle; but the formula is equally applicable to reflexions at different angles,—

1. When both the angles are greater than the polarizing angle.

	θ	Unpolarized Light.
1 reflexion at 58° 2′, and 1 at 67° 2′	0° 34′.	0.0002

2. When one of the angles is above and the other below the polarizing angle.

	θ	Unpolarized Light.
1 reflexion at 53°, and 1 at 58° 2′	0° 12′	0.000024

This experiment requires a very intense light, for I find in my journal that the light of a candle is polarized at 53° and 78°.

In reflexions at different angles, the formula becomes $\tan \theta = \frac{\cos (i + i')}{\cos (i - i')} \times \frac{\cos (I \times I')}{\cos (I \times I')}$, I and i being the angles of incidence. In like manner if a, b, c, d, e, &c. are the values of φ or θ for each reflexion, or rather for each angle of incidence, we shall have the final angle or $\tan \theta = (\tan a \times \tan b) + (\tan c \times \tan d,)$ &c.

It is scarcely necessary to inform the reader that when a pencil of light reflected at 58° 2′ is said to be polarised by another reflexion at 67° 2′, it only means, that this is the angle at which complete polarization takes place in diminishing the angle gradually from 90° to 67° 2′, and that even this angle of 67° 2′ will vary with the intensity of the original pencil, with the opening of the pupil, and with the sensibility of the retina. But when it shall be determined experimentally at what value of φ, or rather at what value of Q, the light entirely disappears from the extraordinary image, we shall be able by inverting the formula to ascertain the exact number of reflexions by which a given pencil of light shall be wholly polarized.

As the value of Q depends on the relation of i to i', that is, on the index of refraction, and as this index varies for the different colours of the spectrum, it is obvious that Q will have different values for these different colours. The consequence of this must be, that in bodies of high dispersive power the unpolarized light which remains in the extraordinary image, and also the light which forms the ordinary image, must be coloured at all incidences; the colours being most distinct near the maximum polarizing angle. This necessary result of the formula, I found to be experimentally true in oil of cassia, and various highly dispersive bodies. In realgar for example φ is $= 0$ at an angle of 69° 0′ for blue light, at 68° 37′ for green light, and at 66° 49′ for red light. Hence there can be no angle of complete polarization for white light, which I also found to be the case by experiment; and as Q must at different angles of incidence have different values for the different rays, the unpolarized light must be composed of a certain portion of each different colour, which may be easily determined by the formula.

Such are the laws which regulate the polarization of light by reflexion from the first surfaces of bodies that are not metallic. The very same laws are applicable to their second surfaces, provided that the incident light has not suffered previous or subsequent refraction from the first surface. The sine of the angle at which φ or Q has a certain value by reflection from the second surface, is to the sine of the angle at which they have the same value at the first surface, as unity is to the index of refraction. Hence φ and Q may be determined by the preceding formulæ after any number of reflexions, even if some of the reflexions are made from the first surface of one body and the second surface of another.

When the second surface is that of a plate with parallel or inclined faces, its action upon light presents curious phenomena, the law of which I have determined. I refer of course to the action of the second surface at angles less than that which produces total reflexion. This action has hitherto remained uninvestigated. It has been hastily inferred, however, from imperfect data; and the erroneous inference forms the basis of some optical laws, which are considered to be fully established.

Among the various results of the preceding investigation, there is one which seems to possess some theoretical importance. If we consider polarized rays as those whose planes of polarization are parallel, then it follows that light cannot be brought into such a state by any number of reflexions, or at any angle of incidence, excepting at the angle of complete polarization. At all other angles the light which seems to be polarized, by disappearing from the extraordinary image of the analysing rhomb, is distinguished from really polarized light, by the property of its planes of polarization forming an angle with each other and with the plane of reflexion. At the polarizing angle, for example, of 56° 45′ in glass, the light reflected is 79.5 rays, and it is completely polarized, because the planes of polarization of all the rays are parallel; but at an angle of incidence of 80°, where 392 rays are reflected, no fewer than 157 appear to be polarized; though their planes of polarization are inclined 66° 26′ to each other, or 33° 13′ to the plane of reflexion. This appearance of polarization, when the rays have only suffered a displacement in their planes of polarization from an angle of 90°, which approximates them to the state of polarized light, arises from the law which regulates the repartition of polarized light between the ordinary and extraordinary images produced by double refraction, and shows that the analysing crystal is not sufficient to distinguish light completely polarized from light in a state of approach to polarization. The difference, however, between these two kinds of light is marked by most distinctive characters, and will be found to show itself in some of the more complex phenomena of interference.

In my paper of 1815, already referred to, I was led by a distant view of the phenomena which I have now developed, to consider common light as composed of rays in every state of positive and negative polarization;* and upon this principle the whole of the phenomena described in this paper may be calculated with the same exactness as upon the supposition of two oppositely polarized pencils. Nothing indeed can be simpler than such a principle. The particles of light have planes, which are acted upon by the attractive and repulsive forces residing in solid bodies; and as these planes must have every possible inclination to a plane passing through the direction of their motion, one-half of them will be inclined — to this plane, and the other half +. When light in such a state falls upon a reflecting surface, the — and the + particles have each their planes of polarization brought more or less into a state of parallelism with the plane of reflexion, in consequence of the action of the repulsive force upon one side or pole of the particle through which the plane passes; while in the particles which suffer refraction, the same sides or poles are by the action of the attractive force drawn downwards so as to increase the inclination of their planes relative to the plane of incidence, and bring them more or less into a state of parallelism with a plane perpendicular to that of refraction.

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* M. Biot has followed me in this opinion. See *Traité de Physique*, tom. iv. p. 304.

The formulæ already given, and those for refracted light which are contained in another paper, represent the laws according to which the repulsive and attractive forces change the position of the planes of polarization; and as we have proved that the polarization is the necessary consequence of these planes being brought into certain positions, we may regard all the various phenomena of the polarization of light by reflexion and refraction, as brought under the dominion of laws as well determined as those which regulate the motions of the planets.

ALLERLY, *December* 25, 1829.

ART. XIX.—ANALYSIS OF SCIENTIFIC BOOKS AND MEMOIRS.

Consolations in Travel, or the Last Days of a Philosopher. By Sir HUMPHRY DAVY, Bart. late President of the Royal Society. London, 1830. 12mo. Pp. 281.

THIS little volume, of which we propose to give some account, is the last production of a philosopher whose brilliant discoveries placed him at the head of chemical science. However exalted be the genius of some of his contemporaries, and however numerous their discoveries, yet Sir Humphry Davy must be allowed to be the Newton of chemistry, and to have sustained the scientific glory of England during his short but illustrious career. With him, and with his two great associates, Dr Wollaston and Dr Young, that glory has departed; and if the nation and the government are not speedily roused from their indifference, we may live to witness the intellectual degradation of England, and to see her science, her philosophy, and her arts, transported to other lands, and directed against the very vitals of her existence.

When Sir Humphry Davy was compelled from ill health to resign the Presidency of the Royal Society, and to abandon those mental exertions which the prosecution of original discovery demanded, the native activity of his mind was directed into other channels of thought less severe upon his debilitated frame, and perhaps more congenial to a mind looking forward to its great deliverance. During his partial recovery from a long and dangerous illness he composed his *Salmonia,* a treatise on angling, interspersed with moral and religious reflections, and adorned with many interesting scientific discoveries of a popular character. The work now under our eye was composed immediately after, under the same unfavourable and painful circumstances, and at a period, as he himself remarks, when the constitution of the author suffered from new attacks. When most other sources of consolation and pleasure were closed to him, he derived a share of both from this exercise of his mind. It was concluded at the very moment of the invasion of his last illness, and it is probable, as the editor observes, that some additions and some changes would have been made had he lived.

The *Consolations in Travel* consists of *Six Dialogues*. 1. The Vision. 2. Discussions connected with the Vision in the Colosseum. 3. The Unknown. 4. The Proteus or Immortality. 5. The Chemical Philosopher. And 6. Pola or Time.

In the first of these dialogues our author gives an account of a vision which he is supposed to have seen while contemplating the ruins of the Colosseum. A spectral being is supposed to appear, and lays before him a brief view of the various states of society from the earliest to the present times. He is then carried up into the Heavens, and has his attention directed to the region of Saturn, whose inhabitants are described as beings possessing organs and powers far more extensive than those of man, which give them an insight into the structure of the universe of which we can form no conception. The first part of the vision is extremely interesting, and will be read with great interest even by those who are best acquainted with the various topics to which it refers; but the latter part of the vision is so completely the work of imagination, and so thoroughly devoid of all probability, that we forbear making any extract from it.

In the third dialogue, called the Unknown, the author and his friends meet at the Ruins of Pæstum with a stranger, who conveys to them much curious information respecting the natural history of the country. The following extract, somewhat abridged, relative to the Lake of Solfatara, will be read with great interest:—

" Ambrosio now came forward, and bowing to the stranger, asked him whether the masses of travertine, of which the Cyclopian walls and the temples were formed, were really produced by aqueous deposition from the river Silaro, as he had often heard reported. The stranger replied;— ' that they were certainly produced by deposition from water; and such deposits are made by the Silaro. But I rather believe, he said, that a lake in the immediate neighbourhood of the city furnished the quarry from which these stones were excavated; and if you like, I will accompany you to the spot from which it is evident that large masses of the travertine, marmor tiburtinum or calcareous tufa have been raised.' We walked to the borders of a lake, on a mass of calcareous tufa, and we saw that this substance had even encrusted the reeds on the shore. There was something peculiarly melancholy in the character of this water; all the herbs around it were grey, as if encrusted with marble. ' There,' said the stranger, ' is, what I believe to be, the source of those large and durable stones which you see in the plain before you. This water rapidly deposits calcareous matter, and even if you throw a stick into it, a few hours is sufficient to give it a coating of this substance. Whichever way you turn your eyes you see masses of this recently produced marble, the consequence of the overflowing of the lake during the winter floods, and in that large excavation you may observe that immense masses have been removed, as if by the hand of art and in remote times;—the marble that remains in the quarry is of the same texture and character as that which you see in the ruins of Pæstum, and I think, it is scarcely possible to doubt, that the builders of those extraordinary structures derived a part of their materials from this spot.' This water is like many, I may say, most of the sources which rise at the foot of the Appennines; it holds carbonic acid in solution which has dissolved a portion of the calcareous matter of the rock through which it has passed;—this carbonic acid is dissipated in the atmosphere, and the marble, slowly thrown down, assumes a crystalline form and produces coherent stones. The lake before us is not particularly rich in the quantity of calcareous matter that it contains, for, as I have found by experience, a pint of it does not afford more than five or six grains; but the quantity of fluid and the length of time are sufficient to account for the immense quantities of tufa and rock which in the course of ages have accumulated in this situation. It can, I think, be scarcely doubted that there is a source of volcanic fire at no great distance from the surface, in the whole of southern Italy; and, this fire acting upon the calcareous rocks of which the Appennines are composed, must constantly detach from them carbonic acid, which rising to the sources of the springs, deposited from the waters of the atmosphere, must give them their impregnation and enable them to dissolve calcareous matter. I need not dwell upon Etna, Vesuvius, or the Lipari islands to prove that volcanic fires are still in existence; and, there can be no doubt, that in earlier periods almost the whole of Italy was ravaged by them; even Rome itself, the eternal city, rests upon the craters of extinct volcanos; and, I imagine that the traditional and fabulous record of the destruction made by the conflagration of Phaeton, in the chariot of the sun, and his falling into the Po, had reference to a great and tremendous igneous volcanic eruption, which extended over Italy and ceased only near the Po at the foot of the Alps. Be this as it may, the sources of carbonic acid are numerous, not merely in the Neapolitan but likewise in the Roman and Tuscan states. The most magnificent waterfall in Europe, that of the Velino near Terni, is partly fed by a stream containing calcareous matter dissolved by carbonic acid, and it deposits marble, which crystallizes, even in the midst of its thundering descent and foam, in the bed in which it falls. The Anio or Teverone, which almost approaches in beauty to the Velino in the number and variety of its falls and cascatelle, is likewise a calcareous water; and there is still a more remarkable one which empties itself into this river below Tivoli, called the lacus Albula, or the lake of the Solfatara. Besides the lake, where the ancient Romans erected their baths, there is another a few yards above it, surrounded by very high rushes and almost hidden by them from the sight. This lake sends down a considerable stream of tepid water to the larger lake, but this water is less strongly impregnated with carbonic acid; the largest lake is actually a saturated solution of this gas, which escapes from it in such quantities in some parts of its surface that it has the appearance of being actually in ebullition. I have found by experiment that the water taken from the most tranquil part of the lake, even after being agitated and exposed to the air, contained in solution more than its own volume of carbonic acid gas with a very small quantity of sulphuretted hydrogen, to the presence of which, I conclude, its ancient use in curing cutaneous disorders may be referred. Its temperature, I ascertained, was in the winter in the warmest parts above 80° of Fahrenheit, and it appears to be pretty constant, for I have found it differ a few degrees only in the ascending source, in January, March, May, and the beginning of June; it is therefore supplied with heat from a subterraneous source, being nearly twenty degrees above the mean temperature of the atmosphere. Kircher has detailed in his *Mundus Subterraneus* various wonders respecting this lake, most of which are unfounded, such as that it is unfathomable, that it has at the bottom the heat of boiling water, and that floating islands rise from the gulf which emits it. It must certainly be very difficult, or even impossible to fathom a source which rises with so much violence from a subterraneous excavation; and, at a time when chemistry had made small progress, it was easy to mistake the disengagement of carbonic acid for an actual ebullition. The floating islands are real, but neither the Jesuit nor any of the writers who have since described this lake, had a correct idea of their origin, which is exceedingly curious. The high temperature of this water, and the quantity of carbonic acid that it contains, render it peculiarly fitted to afford a pabulum or nourishment to vegetable life; the banks of travertine are every where covered with reeds, lichens, confervæ, and various kinds of aquatic vegetables; and, at the same time that the process of vegetable life is going on, the crystallizations of the calcareous matter, which is every where deposited in consequence of the escape of carbonic acid, likewise proceed, giving a constant milkiness to what from its tint would otherwise be a blue fluid. So rapid is the vegetation, owing to the decomposition of the carbonic acid, that even in winter, masses of confervæ and lichens, mixed with deposited travertine, are constantly detached by the currents of water from the bank, and float down the stream, which being a considerable river, is never without many of these small islands on its surface; they are sometimes only a few inches in size, and composed merely of dark-green confervæ or purple or yellow lichens, but they are sometimes even of some feet in diameter, and contain seeds and various species of common water-plants, which are usually more or less incrusted with marble. There is, I believe, no place in the world, where there is a more striking example of the opposition or contrast of the laws of animate and inanimate nature, of the forces of inorganic chemical affinity and those of the powers of life. Vegetables, in such a temperature and every where surrounded by food, are produced with a wonderful rapidity; but the crystallizations are formed with equal quickness, and they are no sooner produced than they are destroyed together. Notwithstanding the sulphureous exhalations from the lake, the quantity of vegetable matter generated there and its heat make it the resort of an infinite variety of insect tribes; and even in the coldest days in winter, numbers of flies may be observed on the vegetables surrounding its banks or on its floating islands, and a quantity of their larvæ may be seen there, sometimes incrusted and entirely destroyed by calcareous matter, which is likewise often the fate of the insects themselves, as well as of various species of shell-fish that are found amongst the vegetables, which grow and are destroyed in the travertine on its banks. Snipes, ducks, and various water-birds often visit these lakes, probably attracted by the temperature and the quantity of food in which they abound; but they usually confine themselves to the banks, as the carbonic acid disengaged from the surface would be fatal to them, if they ventured to swim upon it when tranquil. In May 18— I fixed a stick on a mass of travertine covered by the water, and I examined it in the beginning of the April following, for the purpose of determining the nature of the depositions. The water was lower at this time, yet I had some difficulty, by means of a sharp-pointed hammer, in breaking the mass which adhered to the bottom of the stick; it was several inches in thickness. The upper part was a mixture of light tufa and the leaves of confervæ; below this, was a darker and more solid travertine, containing black and decomposed masses of confervæ; in the inferior part, the travertine was more solid and of a grey colour, but with cavities which I have no doubt were produced by the decomposition of vegetable matter. I have passed many hours, I may say, many days, in studying the phenomena of this wonderful lake; it has brought many trains of thought into my mind connected with the early changes of our globe, and I have sometimes reasoned from the forms of plants and animals preserved in marble in this warm source, to the grander depositions in the secondary rocks, where the zoophytes or coral insects have worked upon a grand scale, and where palms and vegetables now unknown, are preserved with the remains of crocodiles, turtles and gigantic extinct animals of the sauri genus, and which appear to have belonged to a period when the whole globe possessed a much higher temperature. I have likewise often been led from the remarkable phenomena surrounding me in that spot, to compare the works of man with those of nature. The baths, erected there nearly twenty centuries ago, present only heaps of ruins, and even the bricks of which they were built, though hardened by fire, are crumbled into dust, whilst the masses of travertine around it, though formed by a variable source from the most perishable materials, have hardened by time, and the most perfect remains of the greatest ruins in the eternal city, such as the triumphal arches and the Colosseum, owe their duration to this source. Then, from all we know, this lake, except in some change in its dimensions, continues nearly in the same state in which it was described 1700 years ago by Pliny, and I have no doubt contains the same kinds of floating islands, the same plants and the same insects. During the fifteen years that I have known it, it has appeared precisely identical in these respects;—and yet, it has the character of an accidental phenomenon depending upon subterraneous fire. How marvellous then are those laws by which even the humblest types of organic existence are preserved though born amidst the sources of their destruction, and by which a species of immortality is given to generations floating, as it were, like evanescent bubbles, on a stream raised from the deepest caverns of the earth, and instantly losing what may be called its spirit in the atmosphere."

The next extract which we shall submit to our readers relates to the influence of time in producing changes in the material world. After giving an account of the influence of gravitation in producing alterations on the surface of the earth, our author proceeds to consider the *chemical* changes which are produced; and under this head he considers, first, the chemical agency of water, then that of air, and, lastly, that of electricity.

"One of the most distinct and destructive agencies of water depends upon its *solvent* powers, which are usually greatest when its temperature is highest. Water is capable of dissolving, in larger or smaller proportions, most compound bodies, and the calcareous and alkaline elements of stones are particularly liable to this kind of operation. When water holds in solution carbonic acid, which is always the case when it is precipitated from the atmosphere, its power of dissolving carbonate of lime is very much increased, and in the neighbourhood of great cities, where the atmosphere contains a large proportion of this principle, the solvent powers of rain upon the marble exposed to it must be greatest. Whoever examines the marble statues in the British Museum, which have been removed from the exterior of the Parthenon, will be convinced that they have suffered from this agency; and an effect distinct in the pure atmosphere and temperate climate of Athens, must be upon a higher scale in the vicinity of other great European cities, where the consumption of fuel produces carbonic acid in large quantities. Metallic substances, such as iron, copper, bronze, brass, tin and lead, whether they exist in stones, or are used for support or connexion in buildings, are liable to be corroded by water holding in solution the principles of the atmosphere; and the rust and corrosion, which are made, poetically, qualities of time, depend upon the oxydating powers of water, which by supplying oxygen in a dissolved or condensed state enables the metals to form new combinations. All the vegetable substances, exposed to water and air, are liable to decay, and even the vapour in the air attracted by wood, gradually reacts upon its fibres and assists decomposition, or enables its elements to take new arrangements. Hence it is that none of the roofs of ancient buildings more than 1000 years old remain, unless it be such as are constructed of stone, as those of the Pantheon of Rome and the tomb of Theodoric at Ravenna, the cupola of which is composed of a single block of marble. The pictures of the Greek masters, which were painted on the wood of the abies, or pine of the Mediterranean, likewise, as we are informed by Pliny, owed their destruction not to a change in the colours, not to the alteration of the calcareous ground on which they were painted, but to the decay of the tablets of wood on which the intonaco or stucco was laid. Amongst the substances employed in building, wood, iron, tin and lead are most liable to decay from the operation of water, then marble, when exposed to its influence in the fluid form; brass, copper, granite, sienite and porphyry are more durable. But, in stones, much depends upon the peculiar nature of their constituent parts; when the feldspar of the granite rocks, contains little alkali or calcareous earth, it is a very permanent stone; but, when in granite, porphyry or sienite, either the feldspar contains much alkaline matter, or the mica, schorl or hornblende much protoxide of iron, the action of water, containing oxygen and carbonic acid, on the ferruginous elements tends to produce the disintegration of the stone. The red granite, black sienite and red porphyry of Egypt, which are seen at Rome in obelisks, columns and sarcophagi are amongst the most durable compound stones; but, the grey granites of Corsica and Elba are extremely liable to undergo alteration,—the feldspar contains much alkaline matter and the mica and schorl much protoxide of iron. A remarkable instance of the decay of granite may be seen in the hanging tower of Pisa; whilst the marble pillars in the basement remain scarcely altered, the granite ones have lost a considerable portion of their surface, which falls off continually in scales, and exhibits every where stains from the formation of peroxide of iron. The kaolin, or clay, used in most countries for the manufacture of fine porcelain or china, is generally produced from the feldspar of decomposing granite, in which the cause of decay is the dissolution and separation of the alkaline ingredients.

"There are few compound stones, possessing a considerable specific gravity, which are not liable to change from this cause; and oxide of iron amongst *the metallic substances anciently known,* is the most generally diffused in nature, and most concerned in the changes which take place on the surface of the globe. The chemical action of carbonic acid, is so much connected with that of water, that it is scarcely possible to speak of them separately, as must be evident from what I have before said; but the same action which is exerted by the acid dissolved in water is likewise exerted by it in its elastic state, and in this case the facility with which the quantity is changed makes up for the difference of the degree of condensation. There is no reason to believe that the azote of the atmosphere has any considerable action in producing changes of the nature we are studying on the surface; the aqueous vapour, the oxygen and the carbonic acid gas, are, however, constantly in combined activity, and above all the oxygen. And, whilst water, uniting its effects with those of carbonic acid, tends to disintegrate the parts of stones, the oxygen acts upon vegetable matter. And, this great chemical agent, is at once necessary, in all the processes of life and in all those of decay, in which nature, as it were, takes again to herself those instruments, organs and powers, which had for a while been borrowed and employed for the purpose or the wants of the living principle. Almost every thing effected by rapid combinations in combustion, may also be effected gradually by the slow absorption of oxygen; and though the productions of the animal and vegetable kingdom are much more submitted to the power of atmospheric agents than those of the mineral kingdom, yet, as in the instances which have just been mentioned, oxygen gradually destroys the equilibrium of the elements of stones and tends to reduce into powder, to render fit for soils, even the hardest aggregates belonging to our globe. Electricity, as a chemical agent, may be considered, not only as directly producing an infinite variety of changes, but likewise as influencing almost all which take place. There are not two substances on the surface of the globe, that are not in different electrical relations to each other; and, chemical attraction itself seems to be a peculiar form of the exhibition of electrical attraction; and, wherever the atmosphere, or water, or any part of the surface of the earth gains accumulated electricity of a different kind from the contiguous surfaces, the tendency of this electricity is to produce new arrangements of the parts of these surfaces; thus, a positively electrified cloud, acting even at a great distance on a moistened stone, tends to attract its oxygeneous or acidiform or acid ingredients, and, a negatively electrified cloud has the same effect upon its earthy, alkaline, or metallic matter;—and the silent and slow operation of electricity is much more important in the economy of nature than its grand and impressive operation in lightning and thunder. The chemical agencies of water and air, are assisted by those of electricity; and their joint effects combined with those of gravitation and the mechanical ones I first described, are sufficient to account for the results of time. But, the physical powers of nature in producing decay, are assisted likewise by certain agencies or energies of organised beings. A polished surface of a building, or a statue, is no sooner made rough from the causes that have been mentioned, than the seeds of lichens and mosses, which are constantly floating in our atmosphere, make it a place of repose, grow and increase, and from their death, their decay and decomposition carbonaceous matter is produced, and at length a soil is formed, in which grass can fix its roots. In the crevices of walls, where this soil is washed down, even the seeds of trees grow, and, gradually as a building becomes more ruined, ivy and other parasitical plants cover it. Even the animal creation lends its aid in the process of destruction, when man no longer labours for the conservation of his works. The fox burrows amongst ruins, bats and birds nestle in the cavities in walls, the snake and the lizard likewise make them their habitation. Insects act upon a smaller scale, but by their united energies sometimes produce great effect; the ant, by establishing her colony and forming her magazines, often saps the foundations of the strongest buildings, and the most insignificant creatures triumph as it were over the grandest works of man. Add, to these sure and slow operations, the devastations of war, the effects of the destructive zeal of bigotry, the predatory fury of barbarians seeking for concealed wealth under the foundations of buildings and tearing from them every metallic substance,—and it is rather to be wondered, that any of the works of the great nations of antiquity are still in existence.

"The operations of nature, when slow, are no less sure; however man may for a time, usurp dominion over her, she is certain of recovering her empire. He converts her rocks, her stones, her trees into forms of palaces, houses, and ships; he employs the metals found in the bosom of the earth as instruments of power, and the sands and clays which constitute its surface as ornaments and resources of luxury; he imprisons air by water, and tortures water by fire to change or modify or destroy the natural forms of things. But in some lustrums his works begin to change, and in a few centuries they decay and are in ruins; and, his mighty temples, framed as it were for immortal and divine purposes, and his bridges formed of granite and ribbed with iron, and his walls for defence, and the splendid monuments by which he has endeavoured to give eternity even to his perishable remains, are gradually destroyed; and these structures, which have resisted the waves of the ocean, the tempests of the sky and the stroke of the lightning, shall yield to the operation of the dews of Heaven, of frost, rain, vapour and imperceptible atmospheric influences; and, as the worm devours the lineaments of his mortal beauty, so the lichens and the moss, and the most insignificant plants shall feed upon his columns and his pyramids, and the most humble and insignificant insects shall undermine and sap the foundations of his colossal works, and make their habitations amongst the ruins of his palaces and the falling seats of his earthly glory."

Throughout the whole of this interesting volume, we observe traces of the most genuine and unaffected piety, and the most complete proofs, that the author had studied, in his latter days at least, the peculiar doctrines of Christianity, and derived from them that consolation which they are so well fitted to inspire. It is a proud triumph of the Christian faith, that the greatest chemical philosopher of modern times, should not only have added his testimony to its truth, but should have spent his latest hour in impressing his convictions upon others. There perhaps never was an individual who rose more quickly than Sir H. Davy to the highest objects of ambition. Placed in the chair of Newton, at the head of the Royal Society, honoured by the special notice of his sovereign, associated with the highest ranks of society, and distinguished over all Europe, as the most successful of modern inquirers, he yet found that there was something beyond all this, after which his soul aspired, and before which, all earthly glory disappeared.

"Religion," says he, "whether natural or revealed, has always the same beneficial influence on the mind. In youth, in health and prosperity it wakens feelings of gratitude and sublime love, and purifies at the same time that it exalts; but it is in misfortune, in sickness, in age, that its effects are most truly and beneficially felt; when submission in faith, and humble trust in the Divine will, from duties become pleasures, undecaying sources of consolation; then it creates powers which were believed to be extinct, and gives a freshness to the mind, which was supposed to have passed away for ever, but which is now renovated as an immortal hope; then it is the *Pharos* greeting the wave-tossed mariner to his home, as the calm and beautiful still basins or *fiords* surrounded by tranquil groves and pastoral meadows, to the Norwegian pilot escaping from a heavy storm, in the North sea, or as the green and dewy spot gushing with fountains to the exhausted and thirsty traveller in the midst of the desert. Its influence outlives all earthly enjoyments, and becomes stronger as the organs decay and the frame dissolves; it appears as that evening star of light in the horizon of life, which we are sure is to become in another season a morning star, and it throws its radiance through the gloom and shadow of death."

We would strongly recommend this volume, not only to the study of scientific men in general, but especially to those who are just entering upon their philosophical career. At that dangerous period when presumption and scepticism are the attendants of knowledge, it will not be an unprofitable lesson to read in the lives of Newton and of Davy, that in minds of the highest order, humility and piety are the genuine offspring of true science.

ART. XX.—SCIENTIFIC INTELLIGENCE.

I. NATURAL PHILOSOPHY.

ELECTRICITY.

1. *Power of metallic rods or wires to decompose water after their connection with the galvanic pile is broken.*—In the experiments which I undertook in 1806–7, in company with Mr Hisinger, we had found that rods of metal which were employed to decompose water by means of the galvanic pile continued to develope gas after their connection with the pile had ceased,—a circumstance which seemed to indicate a continuance of electrical state, though these rods showed no action upon any other portion of liquid, even of the same kind, than that in which they had been placed during their contact with the pile. This observation, which I had almost forgotten, has been lately confirmed by Pfaff, who has also added to it several others of a similar kind. We might suppose such effects to be produced by a residual polarity, both in the liquid and the metal, showing itself, as long as it continued, by a continuance of chemical action; but some of Pfaff's experiments seem to oppose this idea, for he found that the addition of ammonia to the liquid, by which all its internal polarity was destroyed, did not deprive the wires of their effect. The metals which acquire this property in the highest degree are zinc and iron, next to which is gold. He attempts to explain the phenomenon by supposing that the continued passage of the electrical stream had brought the elements of the water nearer to a state of separation, so that a very slight influence was sufficient to destroy their union. It must be confessed, however, that we cannot at present advance a satisfactory explanation.—Berzelius, *Arsberättelse*, 1829, p. 33.

2. *Detection of alloy in silver by the magnetic needle.*—Oersted has made an ingenious and novel application of the magnetic multiplier. He finds that if a good electro-magnetic multiplier with double needles be suspended by a hair or a thread of unspun silk between two pieces of wrought silver, differing only one per cent. in the quantity of copper they contain; so sensible an effect is produced upon the needle as to render this a more accurate method of proof than the common touch-stones. Small trial plates are made of different degrees of purity, and the piece to be tried is compared with them in the following way: A thin piece of woollen cloth is dipped in muriatic acid, and laid upon the trial plate, after which the piece to be tried is brought into contact with the acid and the wire of the multiplier. The deviation of the needle shows which contains the most alloy, and another trial plate must be employed till the needle cease to be affected, when both are of equal fineness. In coming to a conclusion on this point, however, several circumstances are to be taken into consideration. Wrought silver goods are generally deprived of a portion of their copper by the action of acids, so as to render the surface finer than the inner part of the metal; the proof plates, therefore, must be prepared in the same way. Another source of error in the indications of the needle are the unequal polish and size of the two pieces of metal; the latter of

these is especially difficult to overcome when the surface of the metal to be proved is not plain. When, instead of muriatic acid, a dilute solution of caustic potash is employed, and the result is unlike, it is shown that copper is not the only alloy, but that brass is present; and the potash solution renders that which contains brass so positive, that it seems considerably purer than the trial plate. This is the case also in a very high degree when the alloyed metal contains arsenic, for example when what is called white metal has been used for an alloy. This mode of proof is exceedingly interesting in a scientific point of view, and cases may occur in which it can be employed with advantage; but the sources of error can scarcely be ever so completely done away with as to make it a practical instrument in the hands of the silversmith, as Oersted seems to expect.—Berzelius, *Arsberättelse*, 1829, p. 123.

II. CHEMISTRY.

3. *Double Metallic Chlorides.*—Bonsdorf, in a paper referred to, page 146 of the present number, has described a number of double chlorides of mercury, gold, platinum, and palladium. The following formulæ represent the composition of some of the double chlorides of mercury and platinum. Those of mercury were formed by digesting a cold saturated solution of the salt on the corrosive sublimate in powder.

$$\text{Chlorides of mercury and potassium}\left\{\begin{array}{l} K\,Cl + 4\,Hg\,Cl + 4\,aq. \\ K\,Cl + 2\,Hg\,Cl + 2\,aq. \\ K\,Cl + \,Hg\,Cl + 1\,aq. \end{array}\right.$$

The last of these forms large right rhombic prisms.

Chloride of mercury and sodium = $N\ Cl + 2\,Hg\,Cl + 4\,aq.$

barium = $Ba\ Cl + 2\,Hg\,Cl + 2\,aq.$

$$\text{calcium} = \left\{\begin{array}{l} Ca\ Cl + 5\,Hg\,Cl + 8\,aq. \\ Ca\ Cl + 2\,Hg\,Cl + 6\,aq. \end{array}\right.$$

$$\text{magnesium} = \left\{\begin{array}{l} Mg\,Cl + 3\,Hg\,Cl + \,aq. \\ Mg\,Cl + \,Hg\,Cl + 6\,aq. \end{array}\right.$$

Manganese = $Mn\,Cl + Hg\,Cl + 4\,aq.$

Iron = $Fe\ Cl + Hg\,Cl + 4\,aq.$

In these salts Bonsdorf considers the corrosive sublimate to act the part of an acid which he calls *Acidum Chloro Hydrargyricum.*

Chloride of Platinum and barium = $Ba\ Cl + Pt\,Cl^2 + 4\,aq.$

strontium = $Sr\ Cl + Pt\,Cl^2 + aq.$

calcium = $Ca\ Cl + Pt\,Cl^2 + 8\,aq.$

$$\text{magnesium} = \left\{\begin{array}{l} Mg\ Cl + Pt\,Cl^2 + 6\,aq. \\ Mg\ Cl + Pt\,Cl^2 + 2\,aq. \text{ a yellow powder} \end{array}\right.$$

manganese = $Mn\ Cl + Pt\,Cl^2 + 6\,aq.$

The double salts of platinum with iron, zinc, cadmium, cobalt, nickel, and copper, are isomorphous and identical in composition with that of manganese containing all six atoms water. A great number of other double chlorides of mercury and palladium with the chlorides of the more positive metals were formed, but none of them analyzed.

4. *Freezing point of Alcohol.*—In some observations on this subject in a letter to the editor of the *Annalen de Physik*, xvii. p. 162, Prof. Munckle of Heidelberg, states the following facts:

1. Very good cogniac froze at Melville Island at a natural temperature, according to Parry, of 48°.5 cent.

2. Alcohol of 801 specific gravity at 20° c. at its point of greatest density according to my first experiments at —56°.6 cent., and its probable freezing point is 58° cent.

3. Alcohol nearly absolute of specific gravity 798 froze, according to Hutton, at 79 cent.

4. Absolute alcohol of specific gravity 791 has its point of greatest density, according to my second experiments, at —89.4 cent, and its probable freezing point at 92° cent.

III. NATURAL HISTORY.

MINERALOGY.

5. *Iron Pyrites.*—It is known to mineralogists that common or octohaedral pyrites and the white pyrites, which from their difference of form were considered by Haüy as different species, were found by Berzelius to be identical in composition, or at least that no such difference existed as to warrant their being considered as different species. The explanation then given by Berzelius has been confirmed by later experiments, and he has published the following additional remarks: "When a portion of common pyrites was permitted to fall asunder, I found it to be caused by the formation of a small quantity of protosulphate of iron, which burst asunder the crystallized mass. When the salt was dissolved in water no trace of free sulphur was obtained, from which it appeared, that the efflorescing pyrites contains particles of FeS (sulphuret of iron,) which, changing to the state of salt, tears asunder the rest which undergoes no change. When the small quantity thus changed into sulphate of iron is compared with that which remains unaltered, I did not think that the results of analysis could be obtained to such a degree of accuracy as to determine the matter with certainty. I have since obtained a satisfactory proof of the accuracy of this explanation. I heated carbonate of iron gently in a stream of sulphuretted-hydrogen. There were formed first *sulphuret*, and afterwards *bisulphuret* of iron. The experiment being stopped before all the iron was changed into bisulphuret a pyrite was obtained, which in a few days fell asunder in all directions, and changed into a woolly mass of vitriol of ten times its former volume. Sesqui-sulphuret of iron prepared from the oxide has not this property. It seems, therefore, highly probable, that the falling asunder of the common pyrites arises from the electro-chemical action of the electro-negative bisulphuret upon the sulphuret which is here and there mixed with it in small particles.—Berzel. *Arsberät.* 1829, p. 129.

Kohler finds the specific gravity of common pyrites to vary from 4.826 to 4.837; of the octohaedral from 4.8446 to 4.9074; and that of the cubical to be 4.9188.—Poggend. *An.* xiv. 91.

Art. XXI.—CELESTIAL PHENOMENA,

From July 1st, to October 1st, 1830. Adapted to the Meridian of Greenwich, Apparent Time, excepting the Eclipses of Jupiter's Satellites, which are given in Mean Time.

N. B.—The day begins at noon, and the conjunctions of the Moon and Stars are given in Right Ascension.

JULY.

D.	H.	M.	S.	
4	0	0	0	☿ ☌ ζ ♉
5	0	15		♃ ☍ ☉
5	14	24		○ Full Moon.
8	11	38	5	Em. I. Sat. ♃
9				☿ Greatest Elong.
9	17	52	30	☽ ☌ λ ♒ ☽ 36′ N.
12	15	36		☾ Last Quarter.
13	11			☿ ☌ η ♊
15	13	32	58	Em. I. Sat. ♃
15	16	45	7	☽ ☌ γ ♉ ☽ 41′ N.
15	23	21	4	☽ ☌ α ♉ ☽ 22′ N.
19	12	14		● New Moon.
22	7			♀ ☌ ζ ♉
22	17			☿ ☌ δ ♊
22	22	38		☉ enters ♌
24	9	56	45	Em. I. Sat. ♃
25	9	1	23	Imm. II. Sat. ♃
25	12	48	37	Em. III. Sat. ♃
27	8	36		☽ First Quarter.
29	14			♀ ☌ η ♊
31	11	51	54	Em. I. Sat. ♃
31	19			♅ ☍ ☉

AUGUST.

D.	H.	M.	S.	
1	11	36	17	Em. I. Sat. ♃
4	0	57		○ Full Moon.
4	8	30		☿ Sup. ☌ ☉
6	10	10	11	☽ ☌ φ ♒ ☽ 40′ N.
8	18			☿ ☌ ♄
10	20	8		☾ Last Quarter.
11	22	25	22	☽ ☌ γ ♉ ☽ 33′ N.
11	23			☿ ☌ α ♌
14	15			♄ ☌ ☉
15	10			☿ ☌ ζ ♌
16	10	11	11	Em. I. Sat. ♃
17				☉ Ecl. Invisible.
17	23	53		● New Moon.
18				♂ Stationary.
19	6			☽ ☌ ☿
23	5	7		☉ enters ♍
25	5	40	56	☽ ☌ γ ♌ ☽ 39′ N.
26	2	3		☽ First Quarter.
28	8			☿ ☌ β ♍
30	8	52	47	Em. III. Sat. ♃

SEPTEMBER.

D.	H.	M.	S.	
1	8	30	54	Em. I. Sat. ♃
2				Moon eclipsed, visible
2	8	50		Begins.
2	9	48		Total darkness begins.
2	10	37		Ecliptic ☍
2	10	38		Middle.
2	11	28		End of total darkness.
2	12	26		Eclipse ends.
				21° 40′ digits eclipsed from the N. side of the earth's shadow.
4				♃ Stationary.
5	10	0	5	☽ ☌ ν ♓ ☽ 69′ N.
6	9	34	50	Imm. III. Sat. ♃
6	12	43	14	☽ ☌ μ Ceti ☽ 64′ N.
9	1	58		☾ Last Quarter.
9	9	9	24	Em. IV. Sat. ♃
12	18			☿ ☌ ♄
14	9			☿ ☌ α ♌
16	14	28		● New Moon, ☉ Ecl. Invis.
17				☿ Greatest Elong.
18	12			☿ ☌ α ♍
19	3	15		♂ ☍ ☉
19	15			♀ ☌ ε ♌
23	1	51		☉ enters ♎
24	8	46	13	Em. I. Sat. ♃
24	18	52		☽ First Quarter.
27	8	22	26	Em. II. Sat. ♃
29	1			♄ ☌ α ♌
30	21			☿ Stationary.

Times of the Planets passing the Meridian.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	h	′	h.	′	h	′	h	′	h	′	h	′
JULY.												
1	22	38	21	6	16	43	12	16	2	36	14	7
13	22	35	21	19	16	14	11	21	1	53	13	9
25	23	17	21	25	15	42	10	27	1	10	12	11
AUGUST.												
1	23	51	21	23	15	22	9	56	0	47	11	58
13	0	36	21	49	14	43	9	6	0	7	11	2
25	1	9	22	6	13	58	8	19	23	25	10	7

SEPTEMBER.

D.	h	′	h	′	h	′	h	′	h	′	h	′
1	1	21	22	15	13	29	7	53	23	3	9	57
13	1	32	22	30	12	35	7	10	22	26	9	6
25	1	27	22	43	11	38	6	30	21	48	8	30

Declination of the Planets

JULY.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	°	′	°	′		′	°	′	°	′	°	
1	19	1N.	17	28N.	7	59 S.	22	54 S.	17	0N.	18	30 S.
13	21	38	20	16	6	19	23	4	16	35	18	36
25	22	35	21	57	5	6	23	13	16	8	18	44
					AUGUST.							
1	20	33	22	18	4	37	23	17	15	22	18	49
13	13	20	21	46	4	18	23	23	15	24	18	56
25	4	27	19	45	4	35	23	26	14	55	18	2
					SEPTEMBER.							
1	0	37 S.	17	56	4	59	23	27	14	38	19	6
13	8	19	13	53	5	53	23	28	14	7	19	12
25	13	29	8	53	6	39	23	26	13	42	19	16

ART. XXII.—*Summary of Meteorological Observations made at Kendal in March, April, and May* 1830. By Mr SAMUEL MARSHALL. Communicated by the Author.

State of the Barometer, Thermometer, &c. in Kendal for March 1830.

Barometer.	Inches.
Maximum on the 27th, - - -	30.38
Minimum on the 15th, - - -	29.02
Mean height, - - - -	29.82
Thermometer.	
Maximum on the 29th, - - -	58°
Minimum on the 8th, - -	26.5°
Mean height, - - -	43.17°

Quantity of rain, 5.045 inches.
Number of rainy days, 15.
Prevalent winds, south-west.

The frost, which in the two preceding months had been so severe, has scarcely been experienced in this month. The weather during the greater part of the month, and particularly the latter, has been remarkably mild and genial, and the thermometer has seldom been so low as the freezing point. The equinoctial gales prevailed about the middle of the month with great violence, and were attended with sudden gusts, which are among their distinguishing characteristics. The barometer has mostly been high, but frequently affected by the winds. From the 9th to the 24th the weather was mostly wet and dull. No snow of any consequence has fallen since the 15th, on which day we had frequent hail and snow showers.

April.

Barometer.	Inches.
Maximum on the 27th,	30.00
Minimum on the 24th,	28.75
Mean height,	29.50
Thermometer.	
Maximum on the 30th,	69°
Minimum on the 4th,	23° *
Mean height,	46.30°

Quantity of rain, 5.656 inches.
Number of rainy days, 21.
Prevalent wind, west.

This has been an exceedingly wet month,—at least on many days the showers have been long in continuance, though the quantity when measured has not proved great. We had rain every day, except one, from the 7th to the 27th. At the early part of the month the nights were frosty, but the thermometer has not been so low as the freezing point since the 5th, so that the season has been very favourable in promoting vegetation. It is remarkable that we have not been visited this year by the N. E. and E. winds, which almost uniformly prevail in this and the next month. It is attributed to the unusual mildness of the winter in the northern countries of Europe.

May.

Barometer.	Inches.
Maximum on the 16th,	30.04
Minimum on the 26th,	29.08
Mean height,	29.69
Thermometer.	
Maximum on the 1st,	71°
Minimum on the 4th and 14th	36.5°
Mean height,	51.97°

Quantity of rain, 2.831 inches
Number of rainy days, 14.
Prevalent wind, west.

Though there has been for the most part of the month ungenial weather, arising either from dry easterly winds or showers, yet, from the mean of the thermometer, it does not appear that the air has been so cold as our feelings alone would have led us to conclude. The barometer has been mostly low, and the dry winds which generally visit us much earlier in the season, prevailed for the most part in the E. and N. E. from the 7th to the 14th, and during the day-time occasionally afterwards, and yet the wind has been more in the west than in any other quarter through the month. The quantity of rain for the month is less than might have been expected, but the rain generally fell in drizzling showers, though they were frequent. We have had no frost during the month, but the dry winds about the middle retarded vegetation, though in a less degree than they usually do.

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* N. B. On this day the temperature at Bristol was but 15½°

ART. XXIII.—REGISTER OF THE BAROMETER, THERMOMETER, AND RAIN-GAGE, *kept at Canaan Cottage*. By ALEX. ADIE, Esq. F. R. S. Edin.

THE Observations contained in the following Register were made at Canaan Cottage, the residence of Mr Adie, by means of very nice instruments, constructed by himself. Canaan Cottage is situated about 1½ mile to the south of Edinburgh Castle, about 3 miles from the sea at Leith, and about ¼ of a mile N. of the west end of Blackford Hill. The ridge of Braid Hills is about 1 mile to the south, and the Pentland Hills about 4 miles to the west of south. The height of the instruments is 300 feet above high water-mark at Leith. The morning and evening observations were made about 10 A.M. and 10 P.M.

MARCH 1830.

D. of Week.	Day of Month.	Thermometer.			Register Therm.			Barometer.		Rain.
		Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.	
M.	1	44	47	15.5	31	53	42	29.97	29.99	
T.	2	50	45	47.5	39	54	46.5	29.98	30.50	
W.	3	52	38	45	38	51	46	30.10	30.03	
T.	4	48	33	40.5	31	53	42	29.92	29.72	
F.	5	42	35	38.5	29	46	37.5	29.78	29.79	
S.	6	38	36	37	31	10	35.5	29.94	29.87	
S.	7	34	34	34	32	40	36	29.86	29.76	.36
M.	8	36	43	39.5	29	43	36	29.58	29.27	
T.	9	48	43	45.5	32	53	42.5	29.21	29.38	
W.	10	46	44	45	34	49	41.5	29.23	29.06	.03
T.	11	50	49	49.5	42	55	48.5	29.27	29.13	.12
F.	12	50	35	42.5	47	53	50	29.34	29.44	
S.	13	44	45	44.5	35	49	42	29.71	29.77	
S.	14	45	42	43.5	41	49	45	29.40	29.05	.21
M.	15	38	33	35.5	33	42	37.5	28.77	28.88	.74
T.	16	34	37	35.5	30	39	34.5	29.00	29.10	
W.	17	53	49	51	31	52	41.5	29.25	29.38	
T.	18	55	50	52.5	17	55	51	29.48	29.63	
F.	19	45	39	42	42	47	44.5	29.60	29.71	.17
S.	20	46	38	42	37	50	43.5	29.54	29.87	
S.	21	50	47	48.5	36	54	45	29.89	29.84	
M.	22	47	33	10	38	52	45	29.40	29.36	.15
T.	23	45	46	45.5	34	48	41	29.44	29.50	
W.	24	53	48	50.5	42	56	49	29.63	29.81	
T.	25	52	51	51.5	47	59	53	29.87	30.05	
F.	26	58	42	50	46	62	54	30.22	30.31	
S.	27	53	46	49.5	38	62	50	30.28	30.29	
S.	28	58	56	57	38	64	51	30.17	29.93	
M.	29	58	46	52	53	62	57.5	29.77	29.80	
T.	30	42	40	41	38	46	42	29.88	29.67	
W.	31	42	31	36.5	31	48	39.5	29.58	29.66	
	Sum,	1456	1301	1378.5	1152	15.89	1370.5	919.06	919.85	1.78
	Mean.	46.97	41.97	44.47	37.16	51.26	44.21	29.647	29.673	

APRIL 1830.

D. of Week.	D. of Mon.	Thermometer.			Register Therm.			Barometer.		Rain.
		Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.	
T.	1	34	28	31	26	38	32	29.68	29.68	.06
F.	2	30	30	30	17	36	26.5	29.60	29.48	
S.	3	35	27	31	28	36	32	29.37	29.66	.06
S.	4	35	38	36.5	22	43	32.5	29.87	29.78	
M.	5	47	46	46.5	35	48	41.5	29.60	29.30	
T.	6	50	40	45	43	55	49	29.30	29.43	
W.	7	52	45	48.5	38	58	48	29.40	29.32	
T.	8	52	50	51	44	55	49.5	29.25	29.18	
F.	9	55	43	49	44	63	53.5	29.10	29.13	.31
S.	10	45	43	44	41	46	43.5	29.10	29.20	.09
S.	11	52	39	45.5	37	53	45	29.23	29.17	
M.	12	46	43	44.5	35	57	46	29.07	29.16	.21
T.	13	53	45	49	38	59	48.5	29.50	29.65	.27
W.	14	53	45	49	38	57	47.5	29.50	29.54	.05
T.	15	55	43	49	37	54	45.5	29.11	29.31	
F.	16	56	49	52.5	41	61	51	29.20	29.17	
S.	17	55	45	50	45	59	52	28.97	29.16	
S.	18	53	45	49	42	58	50	29.38	29.40	
M.	19	50	42	46	41	52	46.5	29.25	29.32	.16
T.	20	45	40	42.5	39	53	46	29.35	29.58	
W.	21	50	49	49.5	37	52	44.5	29.35	28.98	.10
T.	22	50	44	47	44	55	49 5	28.98	28.86	.18
F.	23	52	45	48.5	42	59	50.5	28.83	28.71	.51
S.	24	43	40	41.5	42	46	44	28.85	29.39	.30
S.	25	51	50	50.5	37	56	46.5	29.65	29.51	
M.	26	57	50	53.5	47	62	54.5	29.75	29.80	
T.	27	50	57	53.5	47	58	52·5	29.90	29.91	
W.	28	58	50	54	48	68	58	29.87	29.95	
T.	29	57	58	57.5	40	71	55.5	29.78	29.65	
F.	30	64	56	60	42	73	57.5	29.55	29.56	
		1485	1325	1405	1157	1641	1399	881.34	881.97	3.28
		49.5	44.17	46.83	38.57	54.7	46.63	29.378	29.399	

MAY 1830.

D. of Week.	D. of Mon.	Thermometer.			Register Therm.			Barometer.		Rain.
		Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.	
S.	1	58	48	53	46	61	53.5	29.60	29.57	
S.	2	55	45	50	42	59	50.5	29.71	29.86	
M.	3	54	46	50	40	60	50	29.88	29.86	
T.	4	54	49	51.5	43	60	51.5	29.93	29.88	
W.	5	55	49	52	39	61	50	29.88	29.85	
T.	6	56	51	53.5	46	62	54	29.75	28.59	.01
F.	7	54	50	52	42	55	48.5	29.50	29.40	.01
S.	8	44	43	43.5	43	47	45	29.48	29.45	.35
S.	9	43	41	42	41	43	42	29.48	29.51	
M.	10	46	43	44.5	40	48	44	29.60	29.65	
T.	11	46	44	45	40	51	45.5	29.69	29.69	
W.	12	50	45	47.5	36	52	44	29.69	29.75	
T.	13	50	40	45	43	56	49.5	29.87	29.89	
F.	14	57	48	52.5	33	61	47	29.83	29.82	
S.	15	60	50	55	45	64	54.5	29.85	29.88	
S.	16	51	53	52	46	56	51	29.98	29.85	.10
M.	17	58	55	56.5	52	63	57.5	29.82	29.69	
T.	18	55	52	53.5	49	60	54.5	29.60	29.50	.05
W.	19	55	47	51	49	55	52	29.65	29.75	.11
T.	20	55	48	51.5	45	58	51.5	29.75	29.71	
F.	21	52	44	48	45	54	49.5	29.73	29.73	
S.	22	47	46	46.5	42	49	45.5	29.75	29.78	
S.	23	51	48	49.5	43	56	49.5	29.77	29.67	
M.	24	50	48	49	47	50	48.5	29.57	29.33	.55
T.	25	54	50	52	47	55	51	29.10	28.98	
W.	26	53	49	51	47	52	49.5	28.95	29.02	.39
T.	27	54	46	50	46	55	50.5	29.23	29.51	.13
F.	28	50	41	45.5	40	56	48	29.70	29.76	
S.	29	52	50	51	39	54	46.5	29.68	29.35	.06
S.	30	57	48	52.5	47	61	54	29.35	29.21	
M.	31	55	46	50.5	48	59	53.5	29.22	29.26	.20
		1631	1463	1547	1351	1733	1542	908.59	917.78	2.96
		52.61	47.19	49.9	43.55	55.93	49.74	29.309	29.606	

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JOURNAL OF SCIENCE.

ART. I.—*Scientific Men and Institutions in Copenhagen.* By JAMES F. W. JOHNSTON, M. A. Communicated by the Author.

I ENTERED the capital of Denmark on Saturday the 11th of July 1829, and on the Monday following paid an early visit to the University of Copenhagen. The buildings lie detached and scattered about in several adjoining streets in the north-west part of the city. They produce, therefore, no combined effect,—present no hallowed courts of which history can tell that here lived, and taught, and studied the learned men of ancient days, and whose forms the student's fancy can still realize silently flitting through the old archways, or loitering in deep meditation along the secluded cloisters. They have nothing in their exterior, taken thus singly, to awaken any of those undefined, yet peculiar feelings with which we pace the halls and courts of those seats of grave and venerable learning founded by the liberality of our forefathers in our own rich land. Even though united, they would hardly appear imposing, for they are plain brick buildings, void of ornament, and of all attempts at architectural display. A portion of them, the *Studii gaard*, the very core of the university, the seat of the consistory, is almost entirely in ruins, having been burned along with the Fru Kirke, and several adjacent buildings during the bombardment in 1807, and never since repaired. The church, which is the largest in the city, has been only recently re-opened, want of money causing the erection of public buildings in Copenhagen to go on very slowly. It were to be wished that the spire of this church, thrown down at the same time, were again raised to its proper elevation, and the university buildings repaired, as in their present state they serve only to awaken recollections and revive feelings which had much better be forgotten.

The University of Copenhagen was founded by King Christian the 1st, with the permission of the Pope, in the year 1478, and the ordinance by which it was established in its present state is dated May 1788. The number of professors at present is 36, divided into the four usual faculties. Of these three ordinary and two extraordinary belong to the theological, four ordinary and one extraordinary to the law faculty, four ordinary to the faculty of medicine, and nine ordinary and twelve extraordinary to that of philosophy. There is also one lecturer on philology, one upon geology, and two *privatim* teachers of anatomy and animal physiology.*

The ordinary professors have higher rank and emoluments, and are members of the consistory. The term *extraordinary* implies little more than supernumerary. When a man studies with a view to a professorship, he proceeds something after the following manner. He prepares himself, passes the required examinations, &c. and receives the successive degrees of master and doctor in the faculty to which he attaches himself. He is then in a state to give lectures in that faculty, and is therefore called *Doctor Legens*. After obtaining the required permission, he announces his course in the printed catalogues or Index Lectionum, and lectures a year or two for nothing, being a *privatim docens*, or private teacher. If he display sufficient talent, he is then probably appointed a *lector* (lecturer) by Government, with a small salary of perhaps 400 dollars (L. 40.) In course of time a vacancy happens, and he succeeds to a chair, becoming professor extraordinarius, with a salary of about 600 dollars. By-and-bye he succeeds to a seat in the consistorium, and he is then a professor ordinarius, with from 1200 to 1600 dollars, which is the highest. These salaries are derived from various sources, partly from lands, partly from the royal and church tithes, from ground-rents in Copenhagen, from the interest of money, and from the *study money* (studii skatten) paid after a certain rate by all the churches and clergy in the kingdom. Formerly particular lands and other sources of revenue were attached to each chair, and the professors managed them independently, as they thought most for their own benefit. Such a living was called a *corpus*, but in 1796 the *corpora* were *incorporated* into a common fund, from which are now paid all the salaries and other expences of the university.

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* This list is taken from the Index Lectionum for May 1829. The number of professors appointed by the foundation is something less.

In return each professor is "bound to hold public prælections pro officio without payment, over an important part of his science;" and the regulations say further, that "the *lectiones publicæ* shall be given *only* upon that part of the science which is most necessary for all the students, the other and more erudite parts being treated of in the private and *most private* (privatissimis) courses." Yet this would seem not to be so strictly enjoined as to be incapable of invasion, for in the Index Lectionum, dated May 1829, I find Professor Oersted announcing his *public* lectures for the *first Tuesday of every month*—"on the recent discoveries in Experimental Physics,"—and his *private every-day*—on "Experimental Physics,"*—and Professor Zeise, his *public* twice a-week—"on the Elements of the Doctrine of Imponderable Bodies,"—and his *private* three times a-week—"on Chemistry in general,"—to which latter it is added, that they are particularly accommodated to students of medicine and pharmacy, and will be illustrated by experiments.† For these private lectures they are allowed to take fees, but the trifle obtained can scarcely be an object to more than one or two individuals who have large classes. The honararium is limited to four, or where there are experiments to five dollars each course, but all are admitted for nothing who bring a "testimonium paupertatis," as the sons of the clergy generally do. Thus, even those who are rich never pay more than ten dollars a year, or about a pound Sterling. Professor Oersted, whose class amounts to 200, obtains most in this way, and yet his income from fees does not exceed 400 or 500 dollars.

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* Dr Joh. Chr. Oersted, Phys. Prof. Publ. Ord. lectionibus *publicis* primo die Martis cujusque mensis horis 6-8 habendis, inventa recentissima ad Physicam experimentalem spectantia exponere perget; lectionibus *privatis* hora matutina 8-9 physicam experimentalem docebit; nec iis deerit, qui unam alteramve partem physicæ uberius sibi explicatam desideraverint.

† Dr Will. Christophorus Zeise Chemiæ Prof. P. E. *publice* diebus Mercurii et Saturni hora 12—elementa doctrinæ de *Imponderabilibus Chemicorum* exponet; privatim diebus Lunæ, Martis et Veneris eadem hora *Chemiam generalem* lectionibus, Medicinæ et Pharmaciæ Studiosis imprimis accommodatis, delineabit experimentisque, illustrabit: quæ tradita fuerint examinando subinde repetet.

The distribution of subjects for lecture among the different members of the same faculty is not fixed by the statutes of the university. In that of philosophy, where the field is so extensive as to call for some limitation, they are more tied down to certain professions, but the number of professors enables them to choose the department most congenial to their taste. In the other faculties, it is simply provided, as in many of the German universities, that lectures shall be given on certain subjects, and that "the professors shall so arrange their prælections that each department shall be treated of during every Cursus Academicus,"—"and that no collegium or course of lectures shall be extended beyond a year."

Previous to the year 1788 there was only one session and one course of lectures by each professor annually, commencing nearly as in the Scotch universities, at the latter end of the year, and continuing till the middle of summer, but the statute then published announces, that "whereas such a cursus academicus had caused much loss of time, the academical year shall hereafter be divided into two courses, namely, from the first of May to the beginning of October, and from the first of November to the beginning of April." The only holidays at present are, *in summer*, Whitsunday week, the last week in July, and the first week of August,—and *in winter*, fourteen days, from the 24th of December to the 6th of January. Lectures are read on every day of the week during the cursus; and the two recesses, of a month each, are much taken up by the various examinations appointed for those terms. Thus, the professors have very little time at their own disposal, but the king is always willing, due cause being shown, to grant a dispensation from active duties even for entire sessions.

The Polytechnic Institution of Copenhagen, inaugurated in November last, is one of those many institutions to which the rapid advance and diffusion of natural science has given birth. It is confined, as its name implies, to the practical and experimental sciences; and is intended to give a higher and more useful education to those not intended for the learned professions than is to be had in the ordinary schools. The instructions given extend to the higher Algebra, Analytical Geometry, the Differential and Integral Calculus, Pure Mechanics, the science of Machinery, the Mechanical and Chemical part of

Physics, Optics, Agriculture, Chemistry, general, analytical, and as applied to the arts, Technology, Crystallography, Oryctognosy and Geognosy, Botany and Zoology. To teach these sciences seven professors are already appointed, two of whom are chemical, and for practical purposes there are two laboratories, to which all students are admitted under certain regulations, and a work-shop in which, under proper superintendence, practical mechanics is taught.

The students are of two classes, *partial*, those who attend perhaps one or two courses of lectures, and *regular*, who attend all the classes, and go through the regular course assigned by the statutes. The former are admitted without any examination at the rate of four dollars, (8s. 6d English,) a year for each course, and 6s. for the laboratory or work-shop, and thus the popular sciences are open to all at a very moderate charge. The regular students undergo a previous examination in their native tongue,—in French and German, History, Geography, Arithmetic, Geometry, Trigonometry, Algebra as far as Quadratic Equations, and the theory and uses of Logarithms. They pay ten dollars quarterly, or a little more than four guineas a year, for all the benefits of the institution. None of these fees are paid to the professors,—all go to a common fund, from which the total expences are paid, with such aid from government as may be found necessary. The entire *Cursus* occupies two years of two sessions each, after which the regular students undergo the Polytechnic examination. This examination, at the choice of the student, may extend either to the mechanical part, comprising Mathematics and the knowledge of Machinery, Machine Drawing, Mechanics, Chemistry applied to the Arts, and Technology, in which case he becomes, after examination, a *Candidate in Mechanics*, or it may comprise a certain portion of these with all the other branches taught in the institution, in which case he becomes a *Candidate in applied Natural Science*, or he may be examined in all these together, when he has the title in both, (Candidater i Mechaniken og anvendt Naturvidenskab,) and receives his diploma accordingly.

The state of natural science in Copenhagen is not very high, though the University reckons several celebrated men among its scientific professors. Chemistry, botany and zoology, are the favourite studies. Even these, however, can boast of comparatively few cultivators. Here, as in most countries, the greater number, indeed the great body of the students, confine themselves exclusively to what is necessary for passing their examinations, and the knowledge this requires, they may attain either by attending the prælections, or by private study. There are few who cultivate science for its own sake, few amateurs, those who do give themselves up to it having generally a view to the professions. Among a population of less than two millions, by no means rich, many independent cultivators of science are hardly to be looked for, and yet, even here, they contrive always to have a very respectable amount of talent and industrious perseverance collected together in the professional body. There is little inducement to study for a *chair*, and hence a predilection to science may be looked for in those who choose to do so. It may often be remarked of the men of science in England and the north of Europe, that the former are generally more active before they obtain chairs or other lucrative employment, the latter after. The former often work to obtain emolument, the latter, having obtained the means, begin to labour for distinction. I would not be understood by this remark to make any thing like a sweeping assertion against our scientific men, either those who fill public functions or occupy private stations. No country in Europe probably can produce so many examples of private devotion to science as our own: and it is the boast of our public seminaries, that among their professors are ranked some of the first men in Europe in every department. But they who have attended to the history of scientific men in Great Britain will recollect many instances of individuals endowed with rare talents for the promotion of science, who, after making a few successful steps in advancing her interests or enlarging her boundaries, have neglected, or utterly forsaken this noble pursuit, for one that promised a more solid and immediate, and, probably, a more golden harvest.

Professor Oersted, the founder of the science of electro-magnetism, stands at the head of the physical school at Copenhagen. He is a man of 52 years of age, below the middle size, of open and florid complexion, inclining a little to corpulency, and of kind and gentlemanly manners. In conversation he has a habit of looking rather upwards, acquired probably from the peculiarity of his vision, being very near-sighted. He is universally esteemed, not for his talents only, and zeal in the cause of science, which have procured him high respect in foreign countries, but also for his pleasing and amiable disposition. As a writer he was formerly known chiefly for his theoretical and metaphysical papers. Many of the views he then entertained he is said to have abandoned since he began to "interrogate nature for himself." His first experiments on electro-magnetism, when he discovered the action of the galvanic pile on the magnet, were made so late as the winter of 1818-19, and the most complete view of the results then obtained is contained in a short Latin dissertation * of four quarto pages, dated Copenhagen, July 1820.†

When I called on him I found him engaged with his galvanic apparatus on a series of experiments which he proceeded to explain to me without the slightest reserve. There is nothing more gratifying, and few things more useful to a lover of the experimental sciences, than to see the apparatus and witness the manipulations of successful cultivators in their several laboratories. There are so many minutiæ in the forms of apparatus —in the mechanical contrivances for rendering them efficient, or for saving time; and in the modes of manipulation which experience suggests, but which no one would ever think of putting into a book,—that it is now almost necessary for the cultivator of experimental philosophy, who would improve his time, to make himself acquainted with these various matters by direct and personal observation. I paid Oersted several visits; saw all his apparatus, which is entirely modern and in excellent order; and found him on every succeeding visit the more pressing upon me to return. The establishment of the Polytechnic Institution had devolved upon him, and then occupied much of his time; but when not engaged with this affair, I found him always busy either writing or experimenting. He is publishing at present a "*System of Mechanical Philosophy*;" and on showing me some of the proof sheets, he observed, "I am taking a long time to complete it—I get on so slowly, for when I am writing, a new idea comes into my head, and away I go to my experiments."

His motives to exertion may be judged of from his remarks, when speaking of a celebrated lecturer on chemistry in Scotland. " He has every thing in fine order. When I was in Scotland he did me the honour of showing me all his apparatus; but he might do more than give his lectures well."—" Ah but then he makes money, and perhaps thinks that enough."—" Yes, but when a man dies, where is his money !"—" I make honour," he said to me on another occasion, " I make honour my first reward ! Money may come after if it will." He is rather a concise writer, and prides himself a little on this quality of his compositions. " I write," said he to me one day, " as if I were to pay for every line. I know some men who write as if every line were to be paid for."—" I had once a memoir sent to me, which, among many good things, contained also many indifferent." My remark to the author was, " If you will leave out ten dollars worth, it will be worth 100 dollars more !"

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* *Experimenta Circa effectum Conflictus Electrici in Acum Magneticam.*

† Professor Oersted has given the fullest account of his discoveries in the Article THERMO-ELECTRICITY, which he has written in the *Edinburgh Encyclopædia*.—ED.

He proposes to publish a volume of memoirs, comparing with each other the results of metaphysical and experimental philosophy, as soon as his work upon mechanics is fairly out of his hands. Like most other discoverers, he complains of plagiarisms and unjust appropriations by others of what he was the first to make known; but such things to a certain extent can hardly be avoided without an access to foreign publications which naturalists cannot always command, or their knowledge of languages enable them to make use of.

Oersted is secretary to the Academy of Sciences of Copenhagen, and his merits have procured for him the order of Danebroge, and the title of counsellor of state.

The Royal Society of Copenhagen—" *Det Kongelige danske Videnskabers Selskab*"—is the oldest, as it is the most important Philosophical Society in Denmark. It was founded in 1743. There is also a Society for the Encouragement of Experimental Science, but it is of a practical kind, partaking of the character of the " Society for the Diffusion of Useful Knowledge" in this country. The Royal Society has had the direction of two highly important works, a *Complete Survey and Geography of Denmark and the Dukedoms*, and a *Dictionary or Ordbog of the Danish language.* Oersted, as I have already mentioned, is secretary and editor of their yearly Transactions.

Zeise is well known to chemists as the discoverer of Xanthogen and its compounds. He is professor of chemistry (professor extraordinarius) in the university, and has lately been appointed ordinary professor in the Polytechnic Institution. He is a laborious man, but of a melancholy and reserved habit, and shuns rather than courts society. Though he paid me some little attention, I found him, on that account, but an indifferent companion for a stranger, to whom the most communicative person is generally the most acceptable. In his laboratory, which I saw nearly in its perfect state in the new buildings of the Polytechnic Institution, on my second visit to Copenhagen, there was nothing which struck me as worthy of remark. It consisted of several apartments, but all of them seemed to me too small and confined. He is engaged at present with a work on the *Elements of Chemistry*, the first two volumes of which are already published. The last system of chemistry in the Danish language is thirty years old! In the university, therefore, they use French and German works, which the students can all read with ease. In the Polytechnic school it is one of the regulations, that all the lectures shall be after some printed book, either Danish, French, or German, and hence an inducement to write Danish books, as it in some measure insures their sale.

Though chemistry be one of the more popular sciences in Denmark, as it is elsewhere, yet it is but little studied. This may arise partly from the want of a proper lecturer on that department in Copenhagen. Zeise indeed told me he had once or twice given a public course unconnected with the university; but I could not learn that he had been very successful. He has ample science, but will hardly, I should think, make his lectures showy and attractive. In so large a city, a good experimenter, with a popular manner, might surely secure a respectable audience for such an annual course of lectures. Even students of medicine give little attention to it, as the knowledge of chemistry required for degrees in that faculty has not hitherto been very extensive. Zeise's class consists generally of about forty; but as it is a course of chemical pharmacy, it is attended chiefly by those who are educated as apothecaries.

The most diligent and best known chemist in the Danish dominions is Pfaff, of whose labours the German journals show ample proofs. He is besides the author of some chemical and pharmaceutical works of considerable repute; but as Kiel is a German University, and the prælections are delivered in German, his books are also in that language, and therefore not fitted for the islands and states of Denmark proper, for which

Zeise's work is intended. I wished much to have visited Pfaff and the University of Kiel, but the rapid approach of winter on my return to Copenhagen in the end of October prevented me from prolonging my stay in the north of Germany.

Dr Forckhammer is lecturer on geology and mineralogy in the University, and of mineral and inorganic chemistry and analysis in the Polytechnic Institution. He is known for several chemical papers, but has been more lately employed in elucidating the geology of Denmark, with a view to a map of its formations. He has visited the Faroe Islands, and published some account of their geology in the *Transactions of the Danish Academy of Sciences*. In the same work for 1825, he has written upon the structure of a part of Zealand and the neighbouring islands; and more lately (1828) a paper on the Island of Sylt, being the first of a series on the geology of Denmark.

Geology and mineralogy receive little attention in Denmark. There is not even a work on mineralogy in the language. I attended one of Forckhammer's lectures on geology, and found he had only four pupils. In his winter course he has about ten.

There is little inducement, indeed, to study these sciences in that country. The mineral species it produces amount only to about thirty, and the geological formations are still more limited. While Norway remained in possession of the Danes they had a boundless field for both studies. Every mineralogist knows how rich that country is in minerals, and Von Buch has shown it to be no less rich in a geological point of view.

Still in the splendid collections to be found in Copenhagen, there are abundant facilities for the study of mineralogy. To the Museum of Natural History is attached a cabinet of minerals, (King's cabinet) particularly rich in specimens from Greenland, Iceland, Norway, and the Faroe Isles. The silver mines of Kongsberg in Norway have sent to it some splendid masses of native silver. One of these is about six feet long, two feet broad, and eight inches thick. Pieces of the rock are attached to it, but nearly half of the whole is solid silver; and the value of it is said to be 10,000 dollars. Another entirely of pure silver is about two feet long by one foot in breadth and thickness.* This cabinet contains many unexplored treasures; most of the new Greenland and Norwegian minerals having long lain there unknown and neglected. The same is said to be the case also with the collection of minerals belonging to the university. Both indeed are little better than a sealed book—even the professors know little about them; but the persuasion is, that many new things might be detected in them by a careful examination. They have hitherto been under the direction of a professor of the old school, who knew but little of the present state of his science, and, as some are wicked enough to say, cared still less. By a late arrangement, however, he has been relieved from the charge, and it is hoped a year or two will see these cabinets brought into the order they deserve. It is to be regretted, indeed, that minerals collected during years of toil and privation, by such men as Giesecké, should be lost to the world through the incapacity of a professor of mineralogy, whether that incapacity be the result of age or of any other cause. *

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* I did not see these specimens, and copy from a description, which is probably exaggerated.

But the mineralogist will not entirely lose his time at Copenhagen. Besides other private collections, among which must be mentioned that of Conferancerad Monrad, which I had not the pleasure of seeing, he will visit with much gratification the splendid cabinet of the heir-apparent, Prince Christian Frederick. It is under the immediate care and superintendence of Count Vargas Bedemar, and is in the very best order. The Prince and the Count are equally zealous and indefatigable in procuring additions to it, and equally courteous in showing its treasures to the stranger. I spent a portion of two days in it, and I owe to both an acknowledgment of their kind attention. It is arranged according to the system of Haüy, and contains about 10,000 specimens. Were it only displayed in a larger suite of apartments, there would remain nothing to be desired. The Prince has also formed a geological cabinet, and his collection of shells which, from their not interesting me, I did not visit, is said to be as fine and as well kept as his mineral cabinet.

It is probable that practical chemistry and mineralogy will

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* Things would appear to have been better managed in 1808, when Von Buch described the mineral collections of Copenhagen. He praises Professor Wad for being the first to put the royal cabinet in any kind of order, —and feels much pride in numbering so deserving an individual among his brother Wernerians. But twenty-two years have done much for mineralogy, and have rendered necessary a much higher order of talent and information to the scientific arrangement of the productions of the mineral kingdom.

be brought into greater repute, in consequence of the regulations of the Polytechnic Institution. It is there provided that each of the chemical professors shall admit the students into his laboratory twice a-week for three hours in succession. For this purpose they are separated into three divisions, one of which is admitted each day, so that every student has two lessons, or six hours of laboratory-work every week. The fee for this practical course is six dollars, or 12s English.

The zoological part of the Museum of Natural History is under the care of Professor Reinhardt, and is said to have many treasures. Only part of these, however, are yet in a fit state for public inspection. This part consists chiefly of birds and fishes, and is open to the public twice a-week in a house in the Storm-Gaden. This museum is also rich in shells and insects.

Professor Horneman is well known to botanists. He is an old grey-haired man, beneath the middle size, and said to be approaching his eightieth year; yet he is very active and industrious. Professor Zeise was kind enough to accompany me to the botanic garden, connected with which and with the lecture-room is the house in which Horneman resides. Attached to the garden also is a library well stored with botanical works. It occupies a large room over that in which the lectures are delivered. Here we found him with two assistants, all busy among heaps of dried plants. The work at which he has long laboured is the well known *Flora Danica.* It was begun in 1756, during the reign of Frederick the 5th, by both of whose successors it has been liberally patronized and supported. The first ten fasciculi were published by Oeder, who travelled much and laboured indefatigably for the benefit of the work. He was succeeded in the editorship in 1771 by Müller, who, being more devoted to zoology than botany, published only five fasciculi, and these of inferior merit. From 1783, to the beginning of the present century, it was under the care of the celebrated Martin Wahl, author of the well known *Enumeratio Plantarum*, under whose auspices six large fasciculi were added to the former fifteen. In 1804 the work came into the hands of Professor Horneman, who has been equally assiduous. He has published twelve parts, forming four volumes, containing each 180 plates, and describing in all about 900 species of plants. When the work may be completed it is impossible to say. The *Flora* *of Denmark* comprises about 5000* species, of which, though the work has been seventy-four years in progress, and for the greater part of the time indefatigably edited, only 2200 are yet published, so that little more than two-fifths of the labour has yet been performed. "When do you expect to have it finished," I said to Horneman one day. "Ah Monsieur je ne le finirai jamais. Je ne peut pas vivre assez long temps pour cela."—"But how many tomes do you think it will occupy."—"Quinze peut-etre; c'est une chose tres laborieuse de faire un tel ouvrage."

Among the botanists on the continent it is pleasing to hear those of our own country spoken of in such honourable terms. Among cryptogamists the first question asked of me was usually, "Est-ce-que vous connoissez Monsieur Greville à Edinbourg;" for this exceedingly accurate botanist stands deservedly at the head of his department.

We spoke of Dr Hooker: "Oui j'en connois bien, mais je ne l'ai jamais vu;" and he pointed out to me upon his shelves Hooker's *Flora Scotica*, his *Exotic Flora* and *Jungermannia;* and with these Dilwyn's *Confervæ* and Sir J. E. Smith's works. Then, as we went along the garden, he was careful to point out to me a red Potentilla from Nepaul (*Potentilla formosa*) which he received first from Glasgow.

"Mons. Greville," said Horneman, "vous l'avez vu, il est homme grand, n'est ce pas?"—"Yes, he is above the middle size."—"Ah, Je l'ai figure à moi-meme—un homme tres grand."—"How so? Is it that you thought him a great botanist?"—"Peut-etre,—il est grand botaniste sans doute, surtout dans les Cryptogamiques,—dans les Cryptogamiques il est à peu pres parfait." And Dr Hooker, "Je l'imagine d'etre petit."—"But he is a tall man."—"Ah le voila! comment on se trompe de ceux quils n'ayent j'amais vu!"—"And how came you to suppose Dr Hooker a little man?"—"Je ne sais pas."—"But he is an eminent botanist!"—"Ah oui, oui, il est un de vos meilleurs botanistes, mais il ne faut pas d'etre homme grand pour devenir grand homme."—"Still don't you think the chances are in favour of a little man, for the same amount of genius will be more concentrated when it has less space to be diffused over, and don't you see that most clever men are little."—"Oui, oui, cest une bonne idée, et quel malheur ne seroit il pas pour nous trois s'il etoit necessaire d'etre homme grand pour avoir du genie?"

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* 1600 Cotyledonous.
3200 Acotyledonous.

Botany shares, with chemistry, the little attention paid to science in Copenhagen. It is indeed the favourite study in Denmark. It is taught in some of the learned schools; and besides those whose course of study requires them to attend lectures on botany there are also a few who study it as amateurs. I have seen in North Jutland a party of half-a-dozen proceeding along the road with their vasculums slung over their shoulders. But the value set upon it in general does not seem to be very great. "At the lectures which are given *gratis*," said Horneman, "I have perhaps a hundred pupils,—mais quand il faut payer, ma foi! je nai qu' un vingtaine."—"And what is the fee?"—"Cinq dallar!" about 18s. English.

The lecture-room is large, but, as I saw it, dirty and destitute of accommodation,—a simple table and chair in the middle of the room, and a few forms for the pupils, being all it contained. There are about it none of those *agremens* which throw so pleasing an air around the botanical lecture-rooms in Edinburgh and Glasgow. There was also a want of neatness in the keeping of the garden;—it is more a storehouse of plants than an agreeable exposition of the beauties of vegetable nature. Still it is a rich storehouse; and though rather confined, contains about 7000 species. Professor Horneman is himself indefatigable, and I never found him unemployed. If he could inspire his gardeners with a little of his own zeal, the appearance of things would soon alter for the better. The garden is kept up at the expence of government; and, though the low state of the Danish exchequer forbids any lavish expenditure on this department, yet the want of neatness, one would think, cannot be entirely the fault of the authorities.

Though the winters are colder in Copenhagen than with us, the summers are much warmer. Grapes, apricots, and figs all ripen in the open air. The branches of the fig are disengaged from the wall during the winter, stretched out upon the ground and covered up.

Dr Schouw is extraordinary professor of botany. He was educated for the law, but, turning his attention to botany, was first sent out on a tour at the expence of government, and afterwards, by the desire of Horneman, elected professor extraordinarius. The geography of plants is the department to which he has zealously attached himself. He is already favourably known by an excellent work on ancient climate, and he has announced one on the climate of Italy. During both of my visits to Copenhagen he was absent in that country collecting materials.

The mathematical sciences are not much cultivated at this university. Euclid is taught in all the learned schools to the extent of four books,—a work by Dr Ursin, professor of astronomy, being employed as a class-book; but it does not occupy any prominent place among the studies included in the Cursus Academicus. The professors, however, are learned men. The celebrated Schumacher is professor of astronomy, though, from dislike to Copenhagen, he generally resides at Altona. Professor Schmidten also has published several analytical papers in the *Transactions of the Royal Society of Denmark*; and the following *annonce* would imply, that the fault does not lie with him if the higher mathematics are less deeply studied.

M. Henricus Gerner Schmidten, Math. P. P. E. quatuor per hebdomadem diebus, horâ accuratius indicandâ mechanicam puram publice explicabit. Iis præterea, qui unam alteramve analyseos sublimioris partem illustratam voluerint consulere studebit.

PORTOBELLO, 19*th August* 1830.

ART. II.—*Notice of the performance of Steam-Engines in Cornwall for April, May, and June* 1830. By W. J. HENWOOD, F. G. S., Member of the Royal Geological Society of Cornwall. Communicated by the Author.

Reciprocating Engines drawing Water.

Mines.	Diameter of cylinder in inch.	Length of stroke in cylinder in feet.	Length of stroke in the pump in feet.	Load in lbs. per sq. in. of area of piston.	No. of strokes per minute.	Millions of lbs. weight lifted 1 foot high by the consumption of 1 bush. of coal.
Stray Park, -	64	7,75	5,25	7,	4,4	27,5
Huel Vor, -	63*	7,25	5,75	17,5	5,9	27,9
	53	9,	7,5	19,58	5,3	44,8
	48	7,	5,	8,09	5,7	28,3
	80	10,	7,5	14,4	6,	60,2
	45	6,75	5,5	14,3	6,8	47,2
Poladras Downs,	70	10,	7,5	10,1	7,2	59,5
Huel Reeth,	36	7,5	7,5	15,3	3,3	24,4

Mines.	Diameter of cylinder in inch.	Length of stroke in cylinder in feet.	Length of stroke in the pump in feet.	Load in lbs. per sq. in. of area of piston.	No. of strokes per minute.	Millions of lbs. weight lifted 1 foot high by the consumption of 1 bush. of coal.
Balnoon, -	30	8,	7,	9,2	2,8	19,9
Huel Towan, -	80	10,	8,	11,2	5,8	75,6
	80	10,	8,	6,5	4,4	69,4
United Hills, -	58	8,25	6,5	8,6	4,3	46,8
Crinnis, -	56	6,75	6,75	9,1	6,1	41,5
Huel Unity, -	52	6.66	5,75	9,19	6,7	33,6
	60	7,25	5,75	13,8	3,1	36,2
Poldice, -	90	10,	7,	10,54	5,7	48,1
	60	9,5	6,25	12,8	6,5	44,7
Huel Damsel, -	42	7,5	5,75	21,	4,8	33,3
	50	9,	7,	10,1	3,1	35,6
Ting Tang, -	63	8,	6,	15,2	4,7	44,4
	66	9,	7,5	12,6	3,5	54,
Cardrew Downs,	66	8,75	7,	11,67	6,5	62,8
Huel Harmony,	70	9,25	7,	9,68	5,2	37,1
Huel Montague,	50	9,	7,	11,	2,5	23,6
Dolcoath, -	76	9,	7,5	11,87	4,7	42,6
Great Work, -	60	9,	7,	10,25	5,	47,7
Huel Caroline,	30	7,	6,	29,3	7,6	33,1
	53,5	8,33	7,	7,67	5,3	23,2
St. Ives Consols,	36	7,	7,	16,3	6,2	27,
Binner Downs,	70	10,	7,5	10,9	8,5	61,1
	64	9,33	7,75	10,	9,6	55,5
	42	9,	7,5	14.53	7,6	41,9
Consolidated Mines,	90	10,	7,5	8,82	5,5	57,7
	70	10,	7,5	10,92	6,6	62,1
	65	9,	7,5	16,94	2,9	59,5
	90	10,	7,5	8,83	6,9	62,7
	90	10,	7,5	7,3	4,2	39,1
	65	9,	7,5	12,45	5,	64,7
United Mines,	90	9,	8,	7,9	4,3	49,5
	30	9,	7,5	13,66	8,6	48,1
Huel Beauchamp,	36	7,75	6,	13.5	4,4	37,2
Huel Rose, -	60	9,	7,	14,8	6,7	61,9
Pembroke,	80	9,75	7,25	11,34	3,5	51,6
	50	9,	7,	11,58	5,	48,9
East Crinnis, -	60	5,5	5,5	8,57	4,6	25,
	70	10,	7,	9,4	3,6	37,9
East Huel Unity,	45	8,75	6,75	13,15	3,5	36,8
Huel Hope, -	60	9,	8,	11,72	5,7	56,
Huel Tolgus,	70	10,	7,5	8,47	5,5	62,6

Mines.	Diameter of cylinder in inch.	Length of stroke in cylinder in feet.	Length of stroke in the pump in feet.	Load in lbs. per sq. in. of area of piston.	No. of strokes per minute.	Millions of lbs. weight lifted 1 foot high by the consumption of 1 bush. of coal.
Tresavean, -	60	9,	7,	7,67	5,	23,2
Huel Falmouth,	58	8,75	6,5	4,99	4,6	29,2
Huel Sperris, -	70	10,	7,5	7,05	6,3	44,9
Huel Prosper, -	53	7,	7,	4,28	6,	30,8
Huel Leisure, -	36	9,25	6,75	14,5	5,6	36,9
	70	9,833	7,75	6,1	4,9	42,4
Marazion Mines,	60	9,	8,	9,35	6,6	54,
Huel Vor, -	24	6,	3,	12,	16,3	20,7
	27	5,	2,5	12,5	17,5	23,8
	16,5	5,	2,5	8,5	25,8	15,1

Average duty 44,3 millions of lbs. lifted one foot high by the consumption of each bushel (84 lbs.) of coal.

Watt's rotatory double engines, working machinery for bruising tin ores.

Average duty of rotatory double engines, 19.87 millions.

* Watt's double engines. All the other engines are Watt's single.

Art. III.—*Electro-Magnetic Experiments*. By Dr G. Moll, F. R. S. E. M. A. S. Professor of Natural Philosophy in the University of Utrecht. Communicated by the Author.

In the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, Mr Sturgeon of Woolwich, has given a description of an elegant and curious apparatus, with which many striking electro-magnetic experiments may be performed. Among these, is a soft iron wire, bent in the form of a horse-shoe, wound round with copper wire. The ends of this copper wire, being made to communicate with the opposite poles of a galvanic apparatus, the iron becomes a strong horse-shoe magnet, capable of supporting a heavy bar of iron. On lifting the connecting wire out of the cups, the force is immediately destroyed, and again restored on plunging the connecting wire of the battery again in the cups.

This apparatus I saw in 1828 at Mr Watkins's, curator of philosophical apparatus to the London University ; and the horse-shoe with which he performed the experiment became capable all at once of supporting about nine pounds.

I immediately determined to try the effect of a larger galvanic apparatus on a bent iron cylindrical wire, and I obtained results which appear astonishing, and are, as far as the intensity of magnetic force is concerned, altogether new. I have anxiously looked since that time, into different scientific continental and English journals, without finding any further attempt to extend and improve Mr Sturgeon's original experiment.

I procured from Mr Watkins a soft iron wire, bent in the shape of a horse-shoe. The length of the horse-shoe was about 8½ inches, and one inch in diameter. A copper spiral wire was twisted round this iron from right to left (sinistrorsum.) The diameter of this wire was about ⅛ inch, and it was twisted or coiled eighty-three times round the iron. The ends of this wire were made to plunge in cups filled with mercury, in which the connecting wires of the zinc and copper poles of a galvanic apparatus were likewise immersed.

The weight of the horse-shoe, together with its surrounding spiral wire, was about five pounds.* A piece of soft iron, constructed in the same form as the iron which connects the ends or poles of a horse-shoe, or armed magnet, weighed about 1¼ pound, or 630 grammes.

The galvanic apparatus used consisted of one single copper trough, in which a zinc plate was immersed. The acting surface of this zinc plate was about eleven English square feet.

When the conducting fluid was poured into the trough, the horse-shoe immediately became a strong magnet, capable of supporting about twenty-five kilogrammes or fifty pounds.

If weights are added with some caution, this *extempore* magnet may be brought to support seventy-five pounds or thirty-eight kilogrammes.

The south pole of the horse-shoe magnet is on that side on which the copper spiral wire is dipped in the cup connected with the zinc plate ; the north pole, of course, is on the side communicating with the copper trough.

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* The pounds mentioned here, are Dutch weight, of 16 ounces in the pound. This is somewhat heavier than *avoirdupois*. Two pounds are a little less than one kilogramme. But if no great accuracy is required, two pounds may be estimated equal to one kilogramme.

We call the *north pole* of a magnet that end of it which, in a magnetic needle, points to the north.

The rapidity with which such a powerful magnet, capable of supporting seventy-five pounds, is produced, is truly astonishing. With equal celerity the magnetism is destroyed and the poles reversed, merely by shifting the connecting wires of the battery from one cup to another.

The magnetism of this horse-shoe is not, however, instantaneously destroyed, merely by taking the connecting wires out of the cups, and without shifting them from one cup to another. Instead of suspending from the magnet the maximum of what it is able of supporting, if a lesser weight, twenty pounds or ten kilogrammes be attached to it, the magnet will not cease to support the weight, immediately after removing the wires from the cups, but continue to attract the weight for a longer or shorter time, according to the strength of the magnet. The heavier the weight which remains thus suspended, the shorter will be the time after which it falls down.

If the iron bar which so connects both poles of the magnet is supported by the hand, whilst the wires of the battery are removed from one cup to another, the velocity with which the poles are reversed may be actually *felt* by the duration of the impression of the weight on the hand. This duration is certainly much less than one-tenth part of a second.

If we compare the velocity with which the poles are changed, only by transferring the wires from one cup to another, with the trouble and time required to change the poles of a common magnet, capable of supporting 76 pounds, it becomes rather difficult to bring the rule of Horace, *Nil admirari*, in actual practice. If the poles of the horse-shoe are connected, not by the heavy iron rod, or supporter, but by a slender steel needle, the poles may be reversed before the needle by its weight overcomes the resistance of the air. When the wires are shifted, a transient motion is perceptible in the needle, but it does not fall.

When the horse-shoe is loaded till the weight falls, its magnetic force will be found considerably weaker, and some time must elapse before it becomes again capable of carrying the same weight as before. It is well known that common horse-shoe magnets, when allowed to drop their load, are considerably weakened, and often never recover their former strength.

In the experiments which I am now relating, the strongest action of the horse-shoe takes place the instant when the trough is filled and the connection is made.

Although this horse-shoe is only possessed of a transient magnetic force, the duration of which is limited to that of the galvanic action of the battery, it is capable of communicating strong and lasting magnetism to hardened steel bars and compass needles. If a steel bar be rubbed several times from end to end along the poles of the horse-shoe, lasting magnetism is communicated to the bar, exactly as could be effected by any other strong horse-shoe magnet. By touching the horse-shoe in a contrary direction, the magnetism of the bar may be destroyed, or the poles reversed at pleasure. In a similar way, strong magnetism may be impregnated in compass needles, or their poles reversed.

It is a well known fact, that lightning often destroys the action of compasses on ship board; and even there are instances upon record of serious accidents being occasioned by the poles of compass needles being inverted by the electricity of lightning. Prudent navigators provide themselves often, on long voyages, with a set of magnetic bars, by means of which the strength of their compass needles, if impaired or lost, may be restored. But it is possible, nay it is probable, that the same stroke of lightning which destroys the magnetism of the compass needles may also spoil that of the steel bars. A copper trough of modern dimensions, a zinc plate, a little sulphuric and nitric acid, or, if that was considered too dangerous, some sal-ammoniac, and withall a soft iron wire, bent in the shape of a horse-shoe, and wound round with copper wire, would constantly insure the certitude of restoring magnetism on ship board.

After making these experiments, I was anxious to know if the power of the magnet was susceptible of still farther increase by augmenting the strength of the galvanic apparatus. A second trough, the acting superficies of which was about six square feet, was added to the first. The zinc plate of one trough was connected with the zinc of the other, as also the copper of the one with the copper of the other. Thus the superficies brought to action was about seventeen square feet. But the horse-shoe magnet had acquired, it would appear, its maximum of strength, for its magnetic force was not materially increased by this augmentation of galvanic power.

I afterwards tried, with the same view, the powerful galvanic apparatus of Colonel Offerhaus, which I formerly described in Dr Brewster's *Journal.*[*] But no increase of galvanic power was capable of increasing the strength of the magnet beyond a certain limit.

Another iron wire, bent in the shape of a horse-shoe, and similar in every respect to that with which the preceding experiments were made, was surrounded with a spiral coil of *brass* twisted from left to right, (dextrorsum.) The effect was exactly the same as with copper wire, except that the spiral being wound to the right hand side, the north pole was, as might have been anticipated, on the side connected with the zinc.

A brass horse-shoe, wound round either with an iron, a brass, or a copper wire, did not show the least effect. I did not, indeed, expect the least result from this experiment, which was made at the instance of a friend.

It has been shown by the former experiments, that it is of little consequence whether a brass or a copper spiral be used. An iron spiral was tried in its turn, but the precaution was taken of coating the horse-shoe with silk. The iron spiral, wound to the left hand, was $\frac{5}{18}$th inch in diameter; the weight of the apparatus about three kilogrammes, or six pounds, the iron, connecting the ends of the horse-shoe, about $1\frac{1}{4}$ pound. The apparatus thus arranged proved stronger than the former; it supported eighty-six pounds, or forty-three kilogrammes.

Encouraged by these results, I increased the size of the horse-shoe, with a view of investigating whether any considerable augmentation of power might be thus produced. I had a horse-shoe prepared of about $12\frac{1}{2}$ English inches high, and $2\frac{1}{4}$ inches in diameter. A brass spiral of one-eight inch diameter was wound forty-four turns from right to left round this strong bar. The weight of the apparatus was about thirteen kilogrammes, or twenty-six pounds. The connecting bar of the apparatus weighed about four pounds. With an acting galvanic surface of eleven square English feet, the magnet supported sixty-seven kilogrammes, or 135 pounds weight.

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[*] *Edinburgh Philosophical Journal,* t. 6, p. 52.

The horse-shoe was afterwards coated with silk, and an iron spiral wire substituted for that of brass. The apparatus then *supported* 154 *pounds*, but I could not succeed in making it carry an anvil of 200 pounds weight.

It is well known that small magnets, generally speaking, are stronger in proportion to their size than larger ones. I procured a small horse-shoe, coiled round with brass, and weighing in all two pounds. It supported about six pounds.

Vallemont* relates that S. Augustine, was considerably alarmed and terrified, by witnessing some magnetic experiments, amongst which was a magnet supporting several iron rings, suspended from each other. The reverend father does not appear to have been deeply read in Greek philosophers and Latin poets, else he might have known, that the experiment which surprised him so much was known in the days of Plato, and described by Lucretius. S. Augustine would probably have been still more alarmed if he had seen magnets capable of supporting 154 pounds, formed in an instant, and their poles taken away or altered with the velocity of lightning.

The poles of this large magnet were altered, restored, or destroyed with prodigious speed. It proved exceedingly well adapted to communicate a strong magnetism to bars of steel or compass needles.

My next trial was, whether it would be possible to increase the power of a common horse-shoe magnet of hardened steel. A magnet of this description, eight inches and a-half high, weighing about eight pounds, having lost much of its former strength, and capable only of supporting about five pounds, was twisted round with brass wire. It was exposed to galvanic action during several hours, but its strength was not increased in the least degree.

I am far from supposing that the utmost force which I was able to produce by galvanism is the limit of what may be done, and I am continually trying experiments, with a view of increasing the magnetic force already produced. At all events, it appears that the production by galvanism of a magnet capable of supporting 154 pounds, is a curious fact, which a few years ago could be little anticipated.

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* Vallemont, *Description de l'aimant, qui s'en formé à la pointe du clocher neuf de N. D. de Chartres*, p. 164.

I took some pains to look over different books, in order to find accounts of large magnets, either artificial or natural; my trouble, however, was not well rewarded. I found but few and scanty accounts of large magnets.

An old Dutch traveller and painter, *Andrees de Bruÿer*, speaks of an immense natural loadstone, kept in the museum of Florence. Lalande, in his *Voyage en Italie*, gives some further account of this magnet; but it is, as appears, unarmed, and therefore little can be said of its real strength.

One of the largest natural loadstones which I have seen is in the museum of Teyler at Haarlem. It commonly supports 150 pounds weight; but the connecting iron piece, the dish on which the weight stands, &c. may be estimated at least at fifty pounds. Thus the ordinary weight which this loadstone sustains is 200 pounds. But Mr Van Marum asserts, that it is capable of carrying fifty pounds more, without dropping its load. The Teylerian loadstone, therefore, the largest at least in this country, carries 250 pounds.

Another loadstone in the same museum sustains fifteen kilogrammes or 30 pounds.

A loadstone in the collection of the Society Felix Meritis at Amsterdam carries fifty pounds or twenty-five kilogrammes.

The artificial magnets which were made by the Abbé Lenoble were celebrated in their time. The largest weighed nine pounds two ounces, and supported 105 pounds French poids de Marc.

Galileo, in his younger days, applied himself much to the making of magnets; and Castelli, his pupil, speaks of one which weighed only six ounces, and supported fifteen pounds.

Mr Park says* that one of the Emperors of China presented to Toao V. king of Portugal, who reigned from 1750 to 1777, a large natural magnet carrying 200 pounds.

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* Park's *Chemical Catechism*, p. 405.

Our countryman Dr Ingenhouss, made very small artificial magnets, carrying about a hundred times their own weight.

Professor Allamand of Leyden had a magnet, supporting from 80 to 120 pounds. It is now in the collection of the Rotterdam Society of Arts and Sciences.

Coulomb made artificial magnets, weighing ten kilogrammes or twenty pounds, and supporting fifty kilogrammes or 100 pounds.

A certain Keilius or Keil, a German doctor, made, it is said, magnets of extraordinary power, supporting in some cases 250 pounds. A horse-shoe magnet of this man weighed, it is asserted, six pounds, and supported seventy pounds.

It appears from this that the magnet which I made by galvanic force was inferior only to that of the Teylerian Museum, that of the Emperor of China, and to that one which Dr Keilius is said to have made.

Before the discovery of Dr Oerstedt, it was a matter of doubt among natural philosophers, whether any magnetism could be produced by galvanism. Now, magnets nearly equal to the largest in existence are produced instantaneously by the mere application of galvanic power.

When Dr Oerstedt first published his brilliant discovery, it was observed by some that the new facts which were then daily brought to light, added very little to the stock of our knowledge. It was said that these facts were unconnected with each other, and with any others previously known. I am very far from approving these views; and I am much more inclined to believe that the series of new facts discovered within the last years, clearly points out a more intimate connection between phenomena which formerly were held to be entirely independent of each other. Perhaps a few years later, and it will be generally known that many of these disjoined facts are produced by the same general cause.

Since the days of Gilbert, it has been allowed that the earth acts on magnets near its surface, as one magnet on another. Every one knows what is meant by the *magnetism of the earth.* Thus the globe of the earth acts as a system of magnets whose poles are placed in a certain determined position with respect to the terrestrial poles.

But a magnet, when acted on by galvanism, may be made to revolve round its axis. May not, therefore, the revolution of the earth, of that immense magnet, be effected by galvanism by a similar cause?

Magnetic inclination and declination may be produced by galvanism. The magnetic needle is subject to inclination and variation on the surface of the earth. A magnet revolves on its axis when under the influence of galvanism; the earth revolves also on its axis.

The light produced by galvanism is unrivalled by any other artificial light. No argand or any other lamps, or gas light, can be compared to that emitted by charcoal placed between the poles of a large galvanic apparatus. The light of the sun alone is superior to it. Galvanic light in a vacuum will extend itself to greater distances, and its appearance is strikingly similar to that of that light which is observed in the vicinity of the magnetic poles of the earth, of the aurora borealis.

To complete the analogy, the emission of this polar light has an influence on the magnetic needle, which cannot be doubted after the repeated experiments of M. Arago.

Would it be entirely absurd to suppose that aurora borealis is produced in those places where the galvanic force, which determines the rotation of the earth, is communicated to the globe?

No lamp nor gas-light acts on the Bolognese stone, nor on Canton's phosphorus. These bodies are imbibed with light only by exposure to the sun's rays. But the same effect may be produced by exposing the Bolognese stone or Canton's phosphorus to the action of galvanic or electric light. Thus galvanic light alone possesses this analogy with sun-light; and galvanic force is alone capable of producing on a smaller scale the same effects which are dependent on the action of the sun's rays.

Since Mrs Somerville repeated Morrichini's experiments, and pointed out the method of making them with certainty of success, it is scarcely possible to doubt the power of the sun's rays refracted through a prism in magnetizing steel needles. Thus magnetism is alike produced by sun-light, and by galvanic influence. Therefore, it would not appear unreasonable to doubt whether some analogy does not exist between the sun and that force which so strongly affects the magnetic needle.

I need scarcely mention how extended is the range of galvanic action in almost every part of chemical investigation. There is hardly one phenomenon known in chemistry which is not more or less connected with electro-magnetism. In every chemical action, the agency of that force is perceptible, which appears to prevade all nature, and whose influence seems to vivify the mutual action of existing bodies. Thus it would appear that the phenomena of galvanism, far from being disjoined and unconnected with other classes of corpuscular action, may form the links of that chain by which the mutual action of bodies is joined together.

ART. IV.—*On the laws of the polarization of light by refraction.* By DAVID BREWSTER, LL. D. F. R. S. L. and E. *

IN the autumn of 1813 I announced to the Royal Society the discovery which I had then made of the polarization of light by refraction; † and in the November following I communicated an extensive series of experiments which established the general law of the phenomena. During the sixteen years which have since elapsed, the subject does not seem to have made any progress. From experiments indeed stated to have been performed at all angles of incidence with plates of glass, M. Arago announced that the quantity of light which the plate polarized by reflexion at any given angle was equal to the quantity polarized by transmission; but this result, founded upon incorrect observation, led to false views, and thus contributed to stop the progress of this branch of optics.

I had shown in 1813, from incontrovertible experiments, that the action of each refracting surface in polarizing light, produced a physical change on the refracted pencil, and brought it into a state approaching more and more to that of complete polarization. But this result, which will be presently demonstrated, was opposed as hypothetical by Dr Young and the

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* From the *Phil. Trans.* 1830. Read Feb. 25, 1830.

† In this discovery I was anticipated by Malus.

French philosophers; and Mr Herschel has more recently given it as his decision, that of the two contending opinions, that which was first asserted by Malus, and subsequently maintained by Biot, Arago, and Fresnel, is the most probable, —namely, that the unpolarized part of the pencil, in place of having suffered any physical change, retains the condition of common light.

I shall now proceed to apply to this subject the same principles which I have already applied to the polarization of light by reflection, and to establish on the basis of actual experiment the true laws of the phenomena.

The first step in this inquiry is to ascertain the law according to which the polarizing force of the refracting surface changes the position of the planes of polarized light,—a subject which, in as far as I know, has not occupied the attention of any other person.

If we take a plate of glass deviating so slightly from parallelism as to throw off from the principal image the images formed by reflexion from its inner surfaces, we shall be able to see, even at great obliquities, the transmitted light free from all admixture of reflected light. Let this plate be placed upon a divided circle, so that we can observe through it two luminous discs of polarized light A B, Plate II. Fig. 1, formed by double refraction, and having their planes of polarization inclined + 45° and — 45° to the plane of refraction. At an angle of incidence of 0°, when the light passes perpendicularly, the inclination of the planes of polarization will suffer no change; but at an incidence of 30° they will be turned round 40′; so that their inclination to M N or the angle *a e c* will be 45° 40′. At 45° their inclination will be 46° 47′. At 60° it will be 50° 7′; and it will increase gradually to 90°, where it becomes 66° 19°. Hence the maximum change produced by a single plate of glass upon the planes of polarization is 66° 19′ — 45° = 21° 19′, an effect exactly equal to what is produced by reflexion at angles of 39° or 70°. It is remarkable, however, that this change is made in the opposite direction, the planes of polarization now approaching to coincidence in a plane at right angles to that of reflexion. This difference is exactly what might have been expected from the opposite character of the resulting polarization, the poles of the particles of light which were formerly repelled by the force of reflexion, being now attracted by the refracting force.

In this experiment the action of the two surfaces is developed in succession, so that we cannot deduce from the maximum rotation of 21° 19′, the real action of the first, or of a single surface, which must be obviously more than half of the action of the two surfaces, because the planes of polarization have been widened before they undergo the action of the second surface.

In order to obtain the rotation due to a single surface, I took a prism of glass A B C (Fig. 2,) having such an angle B A C, that a ray R R, incident as obliquely as possible, should emerge in a direction R *r* perpendicular to the surface A C. I took care that this prism was well annealed, and I caused the refraction to be performed as near as possible to the vertex A, where the glass was thinnest, and consequently most free from the influence of any polarizing structure. In this way I obtained the following measures.

GLASS.

Angles of Incidence.	Inclination of Planes *ab*, *cd*, (Fig. 1.) to the Plane of Reflexion.	Rotation.
87° 38′	54° 15′	9° 15′
54 50	47 25	2 25
32 20	45 22	0 22

I next made the following experiments with two kinds of glass,—the one a piece of parallel plate glass, and the other a piece of very thin crown. The latter had the advantage of separating the reflected from the transmitted light.

	PLATE GLASS.		CROWN GLASS.	
Incidence.	Inclination.	Rotation.	Inclination.	Rotation.
0°	45° 0′	0° 0′	45° 0′	0° 0′
40	47 28	2 28	47 18	2 18
55	49 35	4 35	49 19	4 19
67	52 53	7 53	52 16	7 16
80	58 53	13 53	58 42	13 42
86½	61 16	16 16	61 0	16 0

I was now desirous of ascertaining the influence of refractive power, although I had already determined in 1813, that a greater quantity of light was polarized, at the same angle of incidence, by plates of a high than by plates of a low refractive power. I experienced great difficulty in this part of the inquiry, from the necessity of having plates without any crystalline structure. I tried gold leaf in a variety of ways; but I found it almost impossible to obtain correct results, on account of the light which was transmitted unchanged through its pores.

By stretching a film of soapy water across a rectangular frame of copper wire I obtained the following measure.

WATER.

Incidence.	Inclination.	Rotation.
85°	54° 17′	9° 17′

I next tried a thin plate of metalline glass of a very high refractive power.

METALLINE GLASS.

Incidence.	Inclination.	Rotation.
0°	45° 0′	0° 0′
20	45 42	0 42
30	46 50	1 50
40	48 0	3 0
55	51 12	6 12
80	62 32	17 32

From a comparison of these results it is manifest that the rotation increases with the refractive power.

In examining the effects produced at different angles of incidence, it becomes obvious that the rotation varies with the deviation of the refracted ray; that is, with $i - i'$ the difference of the angles of incidence and refraction. Hence from a consideration of the circumstances of the phenomena I have been led to express the inclination φ of the planes of polarization to the plane of refraction by the formula,

$$\cot \varphi = \cos (i - i'),$$

the rotation being $= \varphi - 45°$.

This formula obviously gives a minimum at 0°, and a maximum at 90°; and at intermediate points it represents the experiments so accurately, that when the rhomb of calcareous spar is set to the calculated angle of inclination, the extraordinary image is completely invisible—a striking test of the correctness of the principle on which it is founded.

The above expression is of course suited only to the case where the inclination x of the planes of polarization *ab*, *cd*, (Fig. 1,) is 45°; but when this not the case, the general expression is

$$\text{Cot}\ \varphi = \cot x \cos (i - i'.)$$

When the light passes through a second surface, as in a single plate of glass, the value of x for the second surface is evidently the value of φ after the 1st refraction, or in general, calling θ the inclination after any number n of refractions, and φ the inclination after one refraction.

$$\text{Cot}\ \theta = (\cot \varphi)^n$$

When θ is given by observation we have

$$\text{Cot}\ \varphi = \sqrt[n]{\cot \theta}.$$

The general formula for any inclination x and any number n of refractions is

$$\text{Cot}\ \theta = \Big(\cot x \cos (i - i')\Big)^n, \text{ and}$$

$$\text{Cot}\ \varphi = \sqrt[n]{\cot x \cos (i - i'.)}$$

And when $x = 45$ and $\cot x = 1$ as in common light,

$$\text{Cot}\ \theta = \Big(\cos (i - i')\Big)^n.$$

$$\text{Cot}\ \varphi = \sqrt[n]{\cos (i - i'.)}$$

As the term $\Big(\cos (i - i')\Big)$ can never become equal to 0, the planes of polarization can never be brought into a state of coincidence in a plane perpendicular to that of reflexion, either at the polarizing angle, or at any other angle.

In order to compare the formula with experiment, I took a plate of well annealed glass, which at all incidences separates the reflected from the transmitted rays, and in which *m* was nearly 1.510, and I obtained the following results.

Angles of Incidence.	Angles of Refraction.	Rotation observed.	Inclination observed.	Inclination calculated.	Difference.
0°	0° 0′	0° 0′	45° 0′	45° 0′	
10	6 36½	0 13	45 13	45 6	+ 0° 7
20	13 5	0 27	45 27	45 25	+ 0 2
25	16 15	0 32	45 32	45 40	— 0 8
30	19 20	0 40	45 40	46 0	— 0 20
35	22 19	1 12	46 12	46 25	— 0 13
40	25 10	1 30	46 30	46 56	— 0 26
45	27 55	1 42	46 47	47 34	+ 0 47
50	30 29	2 48	47 42	48 24	— 0 42
55	33 52	3 54	48 54	48 59	— 0 5
60	35 0	5 7	50 7	50 36	— 0 29
65	36 53	6 48	51 48	52 7	— 0 19
70	38 29	8 7	53 7	53 59	— 0 52
75	39 45	9 55	54 55	56 18	— 1 23
80	40 42	12 10	57 10	59 5	— 1 55
85	41 17	15 45	60 45	62 24	— 1 39
86	41 21	16 39	61 39	63 9	— 1 30
90	41 28			66 19	

The last column but one of the Table was calculated by the formula,

$$\text{Cot}\ \theta = \left(\cos\,(i - i')\right)^{2}$$

n being in this case 2. The conformity of the observed with the calculated results is sufficiently great, the average difference being only 41′. The errors, however, being almost all negative, I suspected that there was an error of adjustment in the apparatus; and upon repeating the experiment at 80°, the point of maximum error, I found that the inclination was fully 58° 40′, giving a difference only of 25′ in place of 1° 55′. I did not think it necessary to repeat all the observations; but I found, by placing the analyzing rhomb at the calculated inclinations, that the extraordinary image invariably disappeared, the best of all proofs of the correctness of the formula.

In these experiments $x = 45°$ and $\cot x = 1$; but in order to try the formula when x varied from 0° to 90°, I took the case where the angle of incidence was 80° and $\varphi = 58° \, 40'$ when $x = 45°$. The following were the results.

Values of x.	Inclination observed.	Inclination calculated.	Difference.
0°	0° 0′	0° 0′	0° 0′
2½	7 10	7 20	—0 10
5	9 40	8 19	+1 21
10	17 10	16 25	+0 45
15	24 42	24 6	+0 36
20	32 30	31 19	+1 11
25	39 15	37 54	+1 21
30	44 10	43 57	+0 13
35	49 38	49 28	+0 10
40	54 36	54 31	+0 5
45	58 40	59 5	—0 25
50	63 10	63 19	—0 9
55	66 58	67 15	—0 17
60	70 18	70 56	—0 38
65	74 8	74 24	—0 16
70	76 56	77 42	—0 46
75	79 20	80 53	—1 33
80	83 23	83 58	—0 35
85	86 23	86 0	+0 23
90	90 0	90 0	0 0

The last column but one was calculated by the formula $\cot \theta = \cot x . (\cot 58° \, 40')^2$. The differences on an average amount only to 36′.

In determining the quantity of polarized light in the refracted pencil, we must follow the method already explained for the reflected ray, *mutatis mutandis*. The principal section of the analyzing rhomb being now supposed to be placed in a plane perpendicular to the plane of reflexion, the quantity of light Q' polarized in that plane, will be

$$Q' = 1 - 2 \cos^2 \varphi,$$

the quantity of transmitted light being unity. But

$$\text{Cot} \, \varphi = \cot x \cos (i - i',)$$

and as $\cot\varphi = \frac{\cos^2\varphi}{\sin^2\varphi}$ and $\sin^2\varphi + \cos^2 = 1$, we have the quotient and the sum of $\sin^2\varphi$ and $\cos^2\varphi$ to find them. Hence

$$\mathrm{Cos}^2\varphi = \frac{\big(\cot x\cos(i-i')\big)^2}{1+\big(\cot x\cos(i-i')\big)^2}$$

and by substituting this for $\cos^2\varphi$ in the former equation, it becomes

$$Q' = 1 - 2\frac{\big(\cot x\cos(i-i')\big)^2}{1+\big(\cot x\cos(i-i')\big)^2}$$

Now since by Fresnel's formula the quantity of reflected light is

$$R = \tfrac{1}{2}\left(\frac{\sin^2(i-i')}{\sin^2(i+i')} + \frac{\tan^2(i-i')}{\tan^2(i+i')}\right)$$

the quantity of transmitted light T will be

$$T = 1 - \tfrac{1}{2}\left(\frac{\sin^2(i-i')}{\sin^2(i+i')} + \frac{\tan^2(i-i')}{\tan^2(i+i')}\right)$$

Hence

$$Q' = \left(1 - \tfrac{1}{2}\left(\frac{\sin^2(i-i')}{\sin^2(i+i')} + \frac{\tan^2(i-i')}{\tan^2(i+i')}\right)\right)$$
$$\left(1 - 2\frac{\big(\cos(i-i')\big)^2}{1+\big(\cos(i-i')\big)^2}\right)$$

This formula is applicable to common light in which $\cot x = 1$ disappears from the equation; but on the same principles which we have explained in a preceding paper, it becomes for partially polarized rays and for polarized light,

$$Q' = \left(1 - \tfrac{1}{2}\left(\frac{\sin^2(i-i')}{\sin^2(i+i')}\cos^2 x + \frac{\tan^2(i-i')}{\tan^2(i+i')}\sin^2 x\right)\right)$$
$$\left(1 - 2\frac{\big(\cot x\cos(i-i')}{1+\big(\cot x\cos(i-i')\big)^2}\right)$$

In all these cases the formula expresses the quantity of light really or apparently polarized in the plane of refraction.

As the planes of polarization of a pencil polarized + 45° and — 45° cannot be brought into a state of coincidence by refraction, the quantity of light polarized by refraction can never be mathematically equal to the whole of the transmitted pencil, however numerous be the refractions which it undergoes; or, what is the same thing, refraction cannot produce rays truly polarized, that is, with their planes of polarization parallel.

The preceding analysis of the changes produced on common light, considered as represented by two oppositely polarized pencils, furnishes us with the same conclusions respecting the partial polarization of light by refraction, which we deduced in a preceding paper respecting the partial polarization of light by reflexion. Each refracting surface produces a change in the position of the planes of polarization, and consequently a physical change upon the transmitted pencil by which it has approached to the state of complete polarization.

This position I shall illustrate by applying the formula to the experiments which I have published in the *Philosophical Transactions* for 1814.

According to the first of these experiments, the light of a wax candle at the distance of ten or twelve feet is wholly polarized by eight plates, or sixteen surfaces of parallel plate glass at an angle of 78° 52′. Now I have ascertained that a pencil of light of this intensity, will disappear from the extraordinary image, or appear to be completely polarized, provided its planes of polarization do not form an angle of less than 88¾° with the plane of refraction for a moderate number of plates, or 88½° for a considerable number of plates, the difference arising from the great diminution of the light in passing through the substance of the glass. In the present case the formula gives

$$\text{Cot}\,\theta = \Big(\cos(i - i')\Big)^{16} \text{ and } \theta = 88^\circ\ 50'\,;$$

so that the light should appear to be completely polarized, as it was found to be.

At an angle of 61° 0′ the pencil was polarized by 24 plates or 48 surfaces. Here

$$\text{Cot}\,\theta = \Big(\cos\ (i - i')\ \Big)^{48} = 89^\circ\ 36'.$$

At an angle of 43° 34′ the light was polarized by 47 plates or 94 surfaces. Here

$$\text{Cot}\,\theta = \Big(\cos(i - i')\Big)^{94} \text{ and } \theta = 88^\circ\ 27'.$$

It is needless to carry this comparison any further; but it may be interesting to ascertain by the formula the smallest number of refractions which will produce complete polarization. In this case the angle of incidence must be nearly 90°.

Hence $\varphi = 56^\circ\, 29'$ and $\Big(\cos\ (i - i')\Big)^9$ gives 88° 36′, and $\Big(\cos\ (i - i)\Big)^{10}$ 89° 4′; that is, the polarization will be nearly complete by the most oblique transmission through $4\frac{1}{2}$ plates or 9 surfaces, and will be almost perfectly complete through 5 plates or 10 surfaces.

Having thus obtained formulæ for the quantity of light polarized by refraction and reflexion, it becomes a point of great importance to compare the results which they furnish. Calling R the reflected light, these formulæ become

$$Q = R\left(1 - 2\frac{\left(\frac{\cos\,(i + i')}{\cos\,(i - i')}\right)^2}{1 + \left(\frac{\cos\,(i + i')}{\cos\,(i - i')}\right)^2}\right) \text{ and}$$

$$Q' = I - R\left(1 - 2\,\frac{\left(\cos\,(i - i')\ \right)^2}{1 + \left(\cos\,(i - i')\ \right)^2}\right).$$

But these two quantities are exactly equal, and hence we obtain the important general law, that,—At the first surface of all bodies, and at all angles of incidence, the quantity of light polarized by refraction is equal to the quantity polarized by reflection. I have said "of all bodies," because the law is equally applicable to the surfaces of crystallized and metallic bodies, though the action of their first surface is masked or modified by other causes.

It is obvious from the formula that there must be some angle of incidence where $R = 1 - R$, that is, where the reflected is equal to the transmitted light. When this takes place, we have $\sin^2\varphi = \cos^2\varphi'$, that is,

The reflected is equal to the transmitted light, when the inclination of the planes of polarization of the reflected pencil to the plane of reflection, is the complement of the inclination of the planes of polarization of the refracted pencil to the same plane;—or if we refer the inclination of the planes to the two rectangular planes into which the planes of polarization are brought,—The reflected will be equal to the transmitted light when the inclination of the planes of polarization of the reflected pencil to the plane of reflection, is equal to the inclination of the plane of polarization of the refracted pencil to a plane perpendicular to the plane of reflection.

In order to show the connection between the phenomena of the reflected and those of the transmitted light, I have given the following table, which shows the inclination of the planes of polarization of the reflected and the refracted pencil, and the quantities of light reflected, transmitted, and polarized, at all angles of incidence upon glass, *m* being equal to 1.525, and the incident light = 1000.

Angles of Incidence, i.		Angles of Refraction, i'.		Inclination of Plane of Polarization of the Reflected Light, φ'.		Inclination of Plane of Polarization of the Refracted Light, φ.		Quantity of Light Reflected, R.	Quantity of Light transmitted, 1 — R.	Quantity of Light Polarized, Q.
°	′	°	′	°	′	°	′			
0	0	0	0	45	0	45	0	43.23	956.77	0.
2	0	1	18⅔	44	57	45	0.7	43.26	956.74	0.07
10	0	6	32	43	51	45	3	43.39	956.61	1.73
20	0	12	58	40	13	45	13	43.41	956.59	7.22
25	0	16	5	37	21	45	21	43.64	956.36	11.6
30	0	19	8½	33	40	45	31	44.78	955.22	17.24
35	0	22	6	29	8	45	44	46.33	953.67	24.4
40	0	24	56	23	41	46	0	49.10	950.90	32.2
45	0	27	37½	17	22½	46	20	53.66	946.33	44.0
50	0	30	9	10	18	46	45	61.36	938.64	57.4
56	45	33	15	0	0	47	29	79.5	920.5	79.5
60	0	34	36	5	4½	47	54½	93.31	906.69	91.6
65	0	36	28	12	45	48	42	124.86	875.14	112.7
70	0	38	2	18	32	49	28	162.67	837.33	129.8
75	0	39	18	26	52	50	55	257.56	742.44	152.3
78	0	39	54	30	44	51	48	329.95	670.05	157.6
78	7	39	55	30	53	51	50	333.20	666.80	157.65
79	0	40	4	31	59	52	7	359.27	640.73	157.6
80	40	40	13	33	13	52	27½	391.7	608.3	156.7
82	4	40	35	36	22	53	26⅓	499.44	500.56	145.4
84	0	40	42	38	2	53	57	560.32	439.68	134.93
85	0	40	47	39	12	54	22	616 28	383.72	123.7
85	50⅔	40	50⅔	40	12	54	44	666.44	333.56	111.11
86	0	40	51	40	$22\frac{7}{10}$	54	48	676.26	323.74	108.67
87	0	40	54	41	32	55	16	744.11	255.89	89.8
88	0	40	57½	41	23	55	43	819.9	180.1	65.9
89	0	40	58	43	51	56	14	904.81	95.19	36.3
90	0	40	58	45	0	56	29	1000.	0.	0.

It is obvious from a consideration of the principle of the formula for reflected light, that the quantity of polarized light is nothing at 0° because the force which polarizes it is there a minimum. At the maximum polarizing angle, Q is only 79 because the glass is incapable of reflecting more light at that angle, otherwise more would have been polarized. The value of Q then rises to its maximum at 78° 7′, and descends to its minimum at 90°; but the polarizing force has not increased from 56° 45′ to 78° 7′, as the value of φ' shows. It is only the quantity of reflected light that has increased, which occasions a greater quantity of light to disappear from the extraordinary image of the analysing rhomb.

The case, however, is different with the refracted light. The value of Q′ has one minimum at 0° and another at 90°, while its maximum is at 78° 7′; but the force has its minimum at 0° and its maximum at 90°, where its effect is a minimum only because there is no light to polarize. At the incidence of 78° 7′, where the quantities Q, Q′ reach their maxima, the reflected light is exactly one-half of the transmitted light; $\sin^2 \varphi' = \cos^2 \varphi$ and $\tan \varphi' = \cos \varphi$.

At 85° 50′ 40″, where the transmitted light is one-half of the reflected light, the deviation $(i - i') = 45°$, and the quantity of polarized light is one-third of the transmitted light, one-sixth of the reflected light, and one-ninth of the incident light. $\text{Sin}^2 \varphi' : \cos^2 \varphi =$ reflected light : transmitted light, and $\cot \varphi' = \sin (i - i')$.

At 45° we have $(i + i') + (i - i') = 90°$ and $\varphi' = (i - i')$,

$$\text{Tan}\,(i - i') = \frac{\cos\,(i + i')}{\cos\,(i - i')},\ \text{and}\ \tan\,(i - i')^2 = \frac{\left(\sin\,(i - i')\right)^2}{\left(\sin\,(i + i')\right)^2}$$

At 56° 45′, the polarizing angle, the formula for reflected light becomes $R = \frac{1}{2} \left(\sin^2\,(i - i')\right)^2$; but at this angle we have $i' = 90° - i$. Hence we obtain the following simple expression in terms of the angle of incidence, for the quantity of light reflected by all bodies at the polarizing angle.

$$R = \tfrac{1}{2}\,(\cos 2i)^2.$$

I have already mentioned the experiment of M. Arago with plates of glass, in which he found that "at every possible inclination" the quantity of light polarized by transmission was equal to the quantity polarized by reflexion. This conclusion he extends to single surfaces; but it is remarkable that the law is true of single surfaces in which he did not ascertain it to be true, while it is incorrect with regard to plates in which he believes that he has ascertained it to be true. As the consideration of this point does not strictly belong to the present branch of the inquiry, I shall reserve it for a separate communication, "On the action of the second surfaces of transparent plates upon Light."

ALLERLY, *December* 29, 1829.

ART. V.—*On the action of the second surfaces of transparent plates upon light.* By DAVID BREWSTER, LL. D. F. R. S. Lond. and Edin.[*]

IN a paper on the Polarization of Light by Reflexion, published in the *Philosophical Transactions* for 1815, I showed that the Law of the Tangents was rigorously true for the second surfaces of transparent bodies, provided that the sine of the angle of incidence was less than the reciprocal of the index of refraction. The action of the second surfaces of plates at angles of incidence different from the maximum polarizing angle, was studied by M. Arago, who conducted his experiments in the following manner.

"With respect to this phenomenon," says M. Arago, "a remarkable result of experiment may here be noticed; that is, that *in every possible inclination* $A = A'$ [†].

"Let us suppose that a plate of glass E D, Plate II. Fig. 3. is placed in the position that the figure represents before a medium A B of a uniform tint; for instance, a sheet of fine white paper. The eye placed at O, will receive simultaneously the ray I O reflected at I, and the ray B I O transmitted at the same point. Place at *m n* an opaque diaphragm blackened, and perforated by a small hole at S. Lastly, let the eye be furnished with a doubly refracting crystal C, which affords two images of the aperture.

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[*] From the *Phil. Trans.* 1830. Read February 25, 1830.

[†] A is the light polarized by reflexion, and A that polarized by refraction.

"If now, by means of a little black screen placed between B and I, we stop the ray B I which would have been transmitted, the crystal properly placed will give an ordinary image $= A + \frac{1}{2} B$, and an extraordinary image $= \frac{1}{2} B$. But if the screen were placed between A and I, and the ray A I were intercepted, we should still have two images of the hole, and their intensities would be $\frac{1}{2} B'$ and $A' + \frac{1}{2} B'$ respectively. Consequently, without any screen, if the whole of the reflected light A I O, and the transmitted B I O are allowed to arrive at the eye, we shall have for the ordinary image $A + \frac{1}{2} B + \frac{1}{2} B'$, and for the extraordinary image $\frac{1}{2} B + A' + \frac{1}{2} B'$.

"Now it appears from actually making the experiment, that the two images are perfectly equal, *whatever may be the angle formed by the ray* A I *with the plate of glass,* which can only be because A is always equal to A′. Consequently

"The quantity of polarized light contained in the pencil transmitted by a transparent plate, is exactly equal to the quantity of light polarized at right angles, which is found in the pencil reflected by the same plate."

We have no doubt that M. Arago obtained these results, particularly near the polarizing angle, at which limit they are rigorously true; but at all other angles of incidence they are wholly incorrect. When we consider, indeed, the nature of the experiment, which has been lauded for its elegance and ingenuity, we shall see reason to pronounce its results as nothing more than coarse estimates, in which the apparent equality of the two images is the effect either of imperfect observation or of some unrecognized compensation.

If we make the experiment in the manner shown in Fig. 4, with a colourless and well annealed prism of glass E F D, in place of a plate of glass; and make the ray B I enter the surface F D perpendicularly at I, we get rid of all sources of error, and we obtain, what is really wanted, the result for a single surface. In this case the experiment is not disturbed by the light reflected from the inner surfaces of the prism, which is all thrown off from the pencil which enters the eye.

In M. Arago's form of the experiment, part of the ray B I (Fig. 3,) undergoes reflexions within the plate, and there comes along with it to the eye, at O, a portion of light polarized in the plane of reflexion: in like manner the part of the pencil A I that enters the plate, undergoes partial reflexions, and the part reflected from the first surface carries along with it another portion of light polarized in the plane of reflexion, so that four portions of light polarized in the plane of reflexion reach the eye, while only two portions reach it polarized at right angles to the plane of reflexion, viz. those which are polarized by the refraction of each of the surfaces of the plate. Now the part of the pencil A I which suffers a first reflexion from each of the surfaces of the plate, is, as we shall presently show, defective in polarized light compared with that which has experienced two refractions, so that it requires the above additional quantities to produce a compensation with the transmitted pencil B O. If this is not the true cause of the apparent compensation, that is, if M. Arago took means to exclude the reflected pencils which seem to have produced the compensation, we must then ascribe the equality of the two images to inaccuracy of observation.

But even if we admit that M. Arago's experimental results are correct with regard to plates, it necessarily follows that they cannot be true with regard to surfaces; for it is obvious from the slightest consideration of the subject, that the phenomena of the one can never be interchangeable with those of the other.

In order to demonstrate these views by an analysis of the changes which the intromitted light experiences from the two refractions and the intermediate reflexion of a transparent plate, I took a plate of glass of the shape M N (Fig. 5.) having an oblique face M d cut upon one of its ends. A ray of light R A, polarized + 45° and — 45°, was made to fall upon it at A, at an angle of incidence of nearly 83°, so that the inclination of the planes of polarization of the reflected ray A P was about $36\frac{1}{2}$°. Now the ray A C after reflexion in the direction C S, without any refraction at B, where it emerges perpendicularly to Md, would also have had the inclination of its planes of polarization equal to $36\frac{1}{2}$° if there had been no intermediate refraction at A; but this refraction alone being capable of producing an inclination of 53° or a rotation of $53° - 45° = 8°$, and this rotation being in an opposite direction from that produced by the second reflexion at C, the inclination of the planes of polarization for the ray C S is nearly 44½°, the reflexion at C having brought back the ray AC almost exactly into the state of natural light.

Without changing either the light or the angle, I cemented a prism M *c d* on the face M *d*, so that *c d* was parallel to *d* N, and I found that the second refraction at *b*, equal to that at A, changed the inclination of the planes of polarization to 53°; that is, the two refractive actions at A and *b* had overcome the action of reflexion at C, and the pencil *b s* actually contained light polarized perpendicular to the plane of reflexion.

In order to put this result to another test, I took a plate M *c* N Q (Fig. 5.) of the same glass, which separated the pencil *b s* reflected at the second surface, from the parallel pencil A P reflected from the first surface, and I found that at an angle of 83°, the value of the inclination I, or φ, for the ray was about 37½°, while the value of I for the ray *b s* was nearly 55°, an effect almost equal to the refractive action of a plate at 83° of incidence.

When the pencil R A is incident on the first surface at the polarizing angle or 56° 45′, the rotation produced by refraction at A is about 2°, or the inclination $I = 45° + 2° = 47°$; but the maximum action of the polarizing force at C is sufficient to make $I = 0°$ whether x is 45° or 47°. Hence C B is completely polarized in the plane of reflexion, and the refractive action at *b* is incapable of changing the plane of polarization when $I = 0°$: the reason is therefore obvious why the two rotations at A and *b*, of 2° each, produce no effect at the maximum polarizing angle.

If we now call

φ = Inclination to the plane of reflexion produced by the 1st refraction at A,

φ' = Inclination produced by the reflexion at C,

φ'' = Inclination produced by the 2d refraction at *b*,

We shall have

$$\operatorname{Cot} \varphi = \cos (i - i') ; \text{ or } \tan \varphi = \frac{1}{\cos (i - i')}$$

$$\operatorname{Tan} \varphi' = \tan x \left(\frac{\cos (i + i')}{\cos (i - i')}\right) = \frac{\cos (i + i')}{(\cos (i - i'))^2}$$

$$\text{Cot } \varphi'' = \cot x\,(\cos (i - i')) = \frac{(\cos (i - i'))^3}{\cos (\;+ i')}$$

These formulæ are suited to common light where $x = 45°$, but when x varies they become

$$\text{Cot } \varphi = \cot x\,(\cos (i - i')\,)$$

$$\text{Tan } \varphi' = \tan x \left(\frac{\cos (i + i')}{(\cos (i - i'))^2}\right)$$

$$\text{Cot } \varphi'' = \left(\;\cot x \left(\frac{(\cos (i - i'))^3}{\cos (i + i')}\right)\right.$$

Resuming the formula for common light, viz. $\cot \varphi'' = \frac{(\cos (i - i'))^3}{\cos (i + i')}$, it is obvious that when $(\cos (i - i')\,)^3 = \cos (i + i')$, $\cot \varphi'' = 1$, and $\varphi'' = 45°$; that is, the light is restored to common light.

In glass where $m = 1.525$ this effect takes place at 78° 7′; a little below 78° in diamond; and a little above 80° in water.

At an angle below this, φ becomes less than 45°, and the pencil contains light polarized in the plane of reflexion; while at all greater angles φ is above 45°, and the pencil contains light polarized perpendicular to the plane of reflexion. Hence we obtain the following curious law.

"A pencil of light reflected from the second surfaces of transparent plates, and reaching the eye after two refractions and an intermediate reflexion, contains at all angles of incidence from 0° to the maximum polarizing angle, a portion of light polarized in the plane of reflexion. Above the polarizing angle the part of the pencil polarized in the plane of reflexion diminishes till $\cos (i + i') = (\cos (i - i')\,)^3$, when it disappears, and the whole pencil has the character of common light. Above this last angle the pencil contains a quantity of light polarized perpendicularly to the plane of reflexion, which increases to a maximum and then diminishes to zero at 90."

Let us now examine the state of the pencil C S that has suffered only one refraction and one reflexion. Resuming the formula $\tan \varphi' = \frac{\cos (i + i')}{(\cos (i - i'))^2}$, it is evident that when $(\cos (i - i')\,)^2 = \cos (i + i')$, $\varphi' = 45°$, and consequently the light is restored to common light. This takes place in glass at an angle of 82° 44′. At all angles beneath this, the pencil contains light polarized in the plane of reflexion; but at all angles above it, the pencil contains light polarized perpendicular to the plane of reflexion, the quantity increasing from 82° 44′ to its maximum, and returning to its minimum at 90°.

By comparing these deductions with the formula and table for reflected light given in my paper On the Laws of the Polarization of Light by Refraction, the following approximate law will be observed. When

$(\text{Cos}\,(i - i') = \cos\,(i + i')$ All the incident light is reflected.
$(\text{Cos}\,(i - i'))^2 = \cos\,(i + i')$ Half the incident light is reflected.
$(\text{Cos}\,(i - i'))^3 = \cos\,(i + i')$ A 3d of the incident light is reflected.
$(\text{Cos}\,(i - i'))n = \cos\,(i + i')$ An *n*th part of the incident light nearly is reflected.

This law deviates from the truth by a regular progression as *n* increases, and always gives the value of the reflected light in defect. Thus

Angles of Incidence.	Values of *n*.	Differences.
82° 44′	2	0
78 34	3	12
75 38	4	21
68 56	8	38
66 4	11	43
61 22	20	50

Let us now apply the results of the preceding analysis to M. Arago's experiment shown in Fig. 3. Suppose the angle of incidence to be 78° 7′, and let the light polarized by reflexion at A (Fig. 3.) be $= m$, and that polarized by one refraction also $= m$. Then since the pencil *bs* is common light, the polarized light in the whole reflected pencil A P, *bs* is $=$ m, whereas the light polarized by the two refractions is $= 2\,m$; so that M. Arago's experiment makes two quantities appear equal when the one is double that of the other. If the angle exceeds 78° 7′, the oppositely polarized light in the pencil *bs* will neutralize a portion of the polarized light in the pencil A P, and the ratio of the oppositely polarized rays which seem to be compensated in the experiment, may be that of $3m$ or even $4m$ to 1.

Having thus determined the changes which light undergoes by reflexion from plates, it is easy to obtain formulæ for computing the exact quantities of polarized light at any angle of incidence, either in the pencil C B S or *bs*.

The primitive ray R A being common light, A C will not be in that state, but will have its planes of polarization turned round a quantity x by the refraction at A; so that cot x = cos $(i - i'.)$ Hence we must adopt for the measure of the light reflected at C, or the pencil C B S, the formula of Fresnel for polarized light whose plane of incidence forms an angle x with the plane of reflexion. The intensity of A C being known from the formula for common light, we shall call it unity, then the intensity I of the two pencils polarized $-x + x$ to the plane of reflexion will be

$$I = \frac{\sin^2(i-i')}{\sin^2(i+i')}\cos^2 x + \frac{\tan^2(i-i')}{\tan^2(i+i')}\sin^2 x \quad \text{and}$$

$$Q = I\left(1 - 2\frac{\left(\dfrac{\cos(i+i')}{(\cos(i-i'))^2}\right)^2}{1+\left(\dfrac{\cos(i+i')}{(\cos(i-i'))^2}\right)^2}\right) \text{for C B S.}$$

In like manner if we call the intensity of CB = 1, we shall have

$$\text{Tan}\, x = \frac{\cos(i+i')}{(\cos(i-i'))^2}$$

and the intensity I of the transmitted pencil *bs*

$$I = 1 - \frac{\sin^2(i-i')}{\sin^2(i+i')}\cos^2 x + \frac{\tan^2(i-i')}{\tan^2(i+i')}\sin^2 x \text{ and}$$

$$Q = I\left(1 - 2\frac{\left(\dfrac{(\cos(i-i'))^3}{\cos(i+i')}\right)^2}{1+\left(\dfrac{(\cos(i-i'))^3}{\cos(i+i')}\right)^2}\right) \text{for } b\,s.$$

I shall now conclude this paper with the following Table computed from the formulæ in pages 148, 149, and showing the state of the planes of polarization of the three rays A C, CS, and *bs*.

Angle of Incidence on the First Surface.		Angle of Refraction at First Surface, and Angle of Incidence on Second Surface.		Inclination of Plane of Polarization of A C Fig. 5.		Inclination of Plane of Polarization of C S Fig. 5.		Inclination of Plane of Polarization of *bs* Fig. 5.	
°	′	°	′	°	′	°	′	°	′
0	0	0	0	45	0	45	0	45	0
32	0	20	33	45	34	32	20	32	51
40	0	25	10	45	58	24	12	24	56
45	0	27	55	46	17	17	49	18	38
56	30	33	30	47	22	0	0	0	0
67	0	37	34	48	57	18	20	20	50
70	0	38	30	49	33	23	34	27	6
75	0	39	46	50	45	32	22	37	48
78	37	40	29	51	49	38	10	44	59
79	0	40	33	51	56	38	49	45	46
80	0	40	42	52	16	40	27	47	46
83	0	41	5	53	21	44	39	53	40
86	30	41	23	54	47	50	58	60	13
90	0	41	58	56	29	56	29	66	19

ALLERLY, *December* 31*st*, 1829.

ART. VI.—*Abstract of a Meteorological Journal kept at Funchal, in the Island of Madeira, from January* 1*st to December* 31*st*, 1829. By the late C. HEINEKEN, M. D. Commucated by Dr RENTON.

SIR, *Funchal, Madeira,* 11*th March* 1830.

IN compliance with one of the last requests of my lamented friend, Dr Heineken, I beg to forward to you the results of his meteorological observations in Funchal in 1829.

The results of *three years'* observations will be, in Funchal, according to Dr Heineken's tables,

Pressure corrected, Max. 30.406, min. 29.257, mean 30.005.
Temperature, Max. 84, min. 45, mean 65.2.
Dew Point, Max. 77, min. 45.
Dryness, max. 23, min. —

Rain in 1827 = 18.17 in.; 1828 = 17.67 in.; 1829 = 44.44 inches.

This winter has been the most severe recollected by any of the residents. During the night of the 4th ulto. the temperature fell (shown by a minimum thermometer of Newman's hanging on an orange tree in my garden, sheltered by dense foliage above, and by a wall three feet from it in front,) to 44°; and another instrument (same sort and maker,) placed on a board on the ground, its bulb covered with black wool, and exposed, fell on the same night, to 32°. The quantity of rain, too, has been unusally great, and the mountains have been covered with snow at a lower level (little more than 2000 feet above the sea,) and for a much longer time than is generally observed.

A short time ago I received Mr Adie's hygrometer,—a portable and convenient instrument, and, I have no doubt, a generally correct one. During our torrents of rain, however, I was rather surprized to find it show 8 *and* 10° of *dryness* when Daniell's showed *none.* The part of the bulb on which the deposition of moisture ought to take place is in immediate contact with the silk on which the ether is dropped. May not the etherial vapour (displacing, by its superior elasticity, the common atmosphere,) retard deposition on the clear part of the bulb? If a small cup of good ether be placed immediately under the *dark* bulb of Daniell's hygrometer, it will be found, I rather think, on using the instrument in a perfectly still atmosphere, that deposition will not take place so readily as under ordinary circumstances.—I remain, Sir, your very obedient servant,

A. H. RENTON, M. D.

Dr BREWSTER.

1829.

JANUARY.

Pressure	(Inches.)	Cor. for temp.	Temperature			
10 A. M.			10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.29	62 = 30.220	Max.	70	19	
Min.	29.58	60 = 29.501	Min.	48	11	
Mean,	30.00	63 = 29.922	Mean,	60.5	15	65

Dew Point, max. 65, min. 48; *Rain*, 5.87 in.; *Dryness*, max. 16; min. 1.

Winds, N. 13; N. E. 1; E. 2; S. E. 2; W. 3; N. W. 10; = 31 days.

Remarks.—More snow fell, and lower down the mountain than I ever remember, and the thermometer was 4° lower than I had ever seen it. The quantity of rain was moderate, and the barometer by no means so low as one would have expected. It rose considerably *during* the rain. It was the *severest* weather long recollected here; but still it was what in England would be called *fine April weather*. The sun shone every day. It was stormy, showery weather for England; nothing more. One *usual settled winter rain*, although at a higher temperature, is to me more trying and winterly; that is, more like an *English November* while it lasts.

N. B. The lowest min. and mean during three years observation.

FEBRUARY.

Pressure (Inches.) 10 A. M.		Cor. for temp.	Temperature			
			10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.36	69 = 30.264	68	74	17	65
Min.	29.90	64 = 29.817	59	53	9	65
Mean,	30.127	65 = 29.953	62.2	63.5	13	65

Dew Point, max. 68; min. 50; *Rain*, 0.82 in.; *Dryness*, min. 20; max. 0.5.

Mean. of 2 = 62.8.

Winds, N. 2; N. E. 8; E. 5; W. 10; N. W. 3; = 28 days.

Remarks.—A fine clear open month. On the 26th P. M. was an imperfect sirocco, that is, it came from E. not S. E. The sky was cloudless, but not with the peculiar warm haze. It was not in violent puffs, raised the thermometer only moderately, and had not the peculiar parching character of a leste. About noon on 27th, a rim of pale compact clouds or rather mist appeared on the sea horizon. *Here* I take it condensation had taken place, and the true sirocco had ceased, ours being merely the natural atmosphere heated by its contiguity. In the afternoon, clouds appeared on the hills, the portion which had reached us having then become condensed. On the 28th the wind went round to the W. by the S., and the barometer fell, the hills were in a thick mist, and a few drops of rain fell. P. M the wind got to the N. of W., the glass rose; and the sky cleared.

MARCH.

	Pressure (Inches.) 10 A. M.	Cor. for temp.	Temperature			
			10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.09	67 = 29.992	68	71	17	65.5
Min.	29.57	64 = 29.478	57	53	6	65
Mean.	29.907	65 = 29.820	62.5	63.3	11.5	65.3

Dew Point, max. 66; min. 53; *Rain*, 5.74 in.; *Dryness*, max. 13; min. 1.

Mean of 2 = 62.9.

Winds, N. 1; S. W. 1; W. 13; N. W. 16; 31 days.

Remarks.—On the whole a cold wet month. Rain on a greater number of days, (16 in 24.) than I ever remember; but the quantity moderate, (5.74 in.) in proportion to the days; and more showery and little heavy spring rains in a northern climate, than the almost tropical rains which we are accustomed to. From the 27th the wind continued high, with the mountains clear, but the horizon seaward, (S. and S. W.) cloudy; and although the wind was westerly as far as the eye reached, yet from this circumstance, and the gradual rise of the barometer, there seemed to be more north in it in the upper regions, and at a distance.

APRIL.

	Pressure (Inches.) 10 A. M.	Cor. for temp.	Temperature			
			10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.25	66 = 30.169	66	69	13	
Min.	29.85	64 = 29.762	59	54	5	
Mean,	30.01	66 = 29.925	62.5	62	9	65.5

Dew Point, max. 66; min. 52; *Rain*, 3.27 in.; *Dryness*, max. 10; min. 1.

Mean of 2 = 62.2

Winds, N. 8; N. E. 1; S. E. 1; S. W. 2; W. 8; N. W. 10 = 30 days.

Remarks.—A windy, cold, showery month; more like a raw winterly March than April. There has been a longer prevalence of westerly winds this winter and spring than I ever remember. It has been, beyond comparison, the coldest and most trying winter I have experienced here; and different in character from what is usual. Generally the weather at this season is *periodical*, so that you may always calculate upon it before-hand. This year it has defied all guessing even. The barometer has *never* been *very* low, but extremely variable. For the country the weather has, I should think, been favourable, (as mild dry winters and forward springs are peculiarly unfavourable,) and, if succeeded by a warm dry summer, I should anticipate a very large and good vintage.

MAY.

Pressure.	(Inches.)	Cor. for temp.	Temperature.			
10 A. M.			10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.28	66 = 30.199	70	74	16	65.5
Min.	29.85	65 = 29.762	59	55	3	65.5
Mean,	30.137	66 = 30.053	62.5	61.7	10	65.5

Dew Point, max. 67, min. 51; *Rain*, 0.74 in.; *Dryness*, max. 13, min. 2.

Mean of 2 = 62.1

Winds, N. 1; N. E. 23; S. E. 1; W. 2; N. W. 4 = 31 days.

Remarks.—A cold gloomy month for the season.

JUNE.

Pressure.	(Inches.)	Cor. for temp.	Temperature.			
10 A. M.			10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.25	71 = 30.155	69	73	11	
Min.	30.05	69 = 29.952	63	50	55	
Mean,	30.169	71 = 30.073	66.1	65.4	83	65.7

Dew Point, max. 78, min. 55., *Rain*, 2.85 in.; *Dryness*, max. 14, min. 2.

Mean of 2 = 65.7

Winds, N. 3; N. E. 19; S. E. 1; S. W. 1; W. 1; N. W. 5 = 30 days.

Remarks.—A backward dull month. The quantity of rain on the 11th and 12th was unusual.

JULY.

Pressure, 10 A. M.	(Inches.)	Cor. for temp.	Temperature. 10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.28	75 = 30.172	74	76	12.5	66
Min.	30.05	75 = 29.939	69	64	8.5	66
Mean,	30.104	75 = 29.993	71.1	69	10.5	66

Dew Point, max. 74, min. 64; *Rain*, 0 in.; *Dryness*, max. 12, min. 2.

Mean of 2 = 70

Winds, N. E. 29; E. 2 = 31 days.

Remarks—A fine summer month.

AUGUST.

Pressure. 10 A. M.	(Inches.)	Cor. for temp.	Temperature. 10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.25	75 = 30.142	73	75	15	
Min.	30.06	75 = 29.949	68	64	6	
Mean,	30.168	75 = 30.059	70.5	70.8	10.5	66

Dew Point, max. 74, min. 63; *Rain*, 95 in.; *Dryness*, max. 11, min. 1.

Mean of 2 = 70.6

Winds, N. E. 31.

Remarks.—Former part of the month cold, cloudy, and unseasonable; the heavy rains most unusual.

SEPTEMBER.

Pressure. 10 A. M.	(Inches.)	Cor. for temp.	Temperature. 10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.30	76 = 30.189	73	75.5	14.5	
Min.	29.90	74 = 29.789	67	62	5.5	
Mean,	30.116	74 = 30.049	70	69	10	66.5

Dew Point, max. 72; min. 59; *Rain*, 0.04 in.; *Dryness*, max. 17; min. 2.

Mean of 2 = 69.5

Winds, N. E. 25; N. 4; N. W. 1 = 30 days.

Remarks.—Weather unusually cloudy at the beginning of the month, but, on the whole, it has been a pleasant one. The few drops of rain that fell in Funchal did not affect the rain-gage. A slight earthquake was felt on the evening of the 16th, and another equally slight and partial at 4 A. M. of the 18th. The summer has been the mildest, *i. e.* the coolest, I have experienced in the island.

OCTOBER.

Pressure. (Inches.) 10 A. M.		Cor. for temp.	Temperature. 10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.24	73 = 30.129	70	73	15	
Min.	29.97	74 = 29.859	64	58	2	
Mean,	30.14	72 = 30.036	64	63.5	85	66.8

Dew Point, max. 71, min. 54; *Rain*, 1.72 in.; *Dryness*, max. 18, min. 0.

Mean of 2 = 63.7

Winds, N. E. 26; N. 3; N. W. 1; S. W. 1 = 31 days.

Remarks.—A fine month. The rain at the end of it very acceptable.

NOVEMBER.

Pressure. (Inches.) 10 A. M.		Cor. for temp.	Temperature. 10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.19	70 = 30.092	70	73	21	
Min.	29.43	65 = 29.347	58	52	2	
Mean,	29.80	65 = 29.715	65.2	63.9	11.5	

Dew Point, max. 70, min. 55; *Rain*, 13.68 in.; *Dryness*, max. 10, min. 0.

Mean of 2 = 64.5

Winds, S. W. 3; W. 11; N. 3; N. E. 2; S. E. 2; S. 1; N. W. 8 = 30 days.

Remarks.—A boisterous wintry month; the ships in the roads driven out to sea, and kept out for nearly a fortnight. Two wrecked on the coast. The rain however is considered very seasonable by the country people. A great deal of thunder and lightning during the latter half of the month.

DECEMBER.

Pressure (Inches.) 10 A. M.		Cor. for temp.	Temperature			
			10 A. M. and P. M.	Max. & Min.	Range.	Well.
Max.	30.29	64 = 30.205	64	66	11	65
Min.	29.58	62 = 29.503	55	50	4	65
Mean,	30.03	62 = 29.952	59.9	58.7	7.5	65

Dew Point, max. 64; mean. 52; *Rain,* 8.76 in.; *Dryness,* max. 10; min.

Mean, 59.3.

Winds, N. 3; N. E. 15; W. 5; S. W. 3; S. 1; N. W. 4; = 31 days.

Remarks.—A cold changeable and disagreeable month. Thunder and lightning of almost daily occurrence.

ANNUAL RESULTS. (1829.)

	Pressure.	Cor. for temp.
Max.	30.264	76
Min.	29.347	48
Mean	29.962	64.5

Dew Point, max. 74; min. 48; *Rain,* 44.44 in.; *Dryness,* max. 20; min.

Winds, N. 41; N. E. 180; E. 9; S. E. 7; W. 53; N. W. 62; S. W. 11; S. 2; = 165 days.

ART. VII.—*Account of a Fifth Case of Spectral Illusion.* Continued from Art. xix p. 319, of No. iv. New Series.

I AM sorry to say that the spectral illusions which I have described to you do not appear to have yet terminated, as you will see by the continuance of my relation.

I have been from home about a fortnight. During my absence Mrs ——— informs me that she has frequently heard me moving near her. Nearly every night as she lay awake she distinctly heard sounds like my breathing hard on the pillow by her bed side, and other sounds such as I might make by turning in bed, &c. On one occasion, while riding with a neighbour, Mr ——— and another lady, she heard my voice frequently as if I were riding by her side, she heard also the sound of my horse's feet, and was almost puzzled by hearing me address her at the same time with the persons really in company. My voice made remarks on the scenery, improvements, &c. such as I probably should have done had I been there. There was no visible apparition.

Last night Mrs ——— was preparing for bed, having dismissed her maid, and was sitting with her feet in hot water, and, as she tells me, thinking on, and even repeating to herself (having an excellent memory) a striking passage she had been reading in the last *Edinburgh Review*, when on raising her eyes she saw sitting in a large easy chair before her, the figure of a departed friend (my sister). She appeared dressed, as was usual with her, with great neatness, but in a gown of a peculiar kind, such as Mrs ——— had never seen her wear, but exactly such as had been described by her by a common friend, as having been worn by my sister during her last visit to England. Mrs ——— paid particular attention to the dress, air, and appearance of the figure, which sat in an easy attitude in the chair, holding a handkerchief in one hand. Mrs ——— tried to speak to it, but felt a difficulty in doing so, and in about three minutes the figure disappeared. I came into the room about a minute afterwards, and found Mrs ——— slightly nervous, but fully aware of the delusive nature of the apparition. She described it as having all the vivid colouring and apparent reality of life. Mrs ——— had passed a week without her usual tonic. She describes a peculiar feeling in the eyes as preceding for some hours these visions, which sensation appears to be relieved after the vision is over.

March 18*th*, 1830.

gin to the deposition of that rock. Looking to the period when the secondary rocks were the latest in existence, the primæval ocean must have washed the Apennines near Capua, fourteen miles directly landward from the present shore, and the curve of the bay extended from its present eastern point, which is composed of Apennine limestone, to a promontory to the westward, which could not have been nearer than the present Cape of Mondragone, where the limestone has assumed beautiful tints, and is quarried for ornamental purposes. The bay must then have had double its present aperture. The point of Minerva, which is the eastern termination, was formerly in all probability contiguous to the island of Capri, which has been severed by the abrading influence of the current now setting through the strait.

This fine island, which has hitherto been barely mentioned, is extremely different in appearance and character from the other islands of the vicinity. It is composed almost entirely of limestone, which, rising abruptly from the sea, forms an eminence extremely conspicuous from the town of Naples, and the whole island forms a break-water in the mouth of the Bay, which gives it a far more compact form than it otherwise would have had, which induced its ancient name of *Crateris Sinus.* Those who have visited Capri, (which I was accidentally prevented from doing,) describe it as a highly picturesque, and in some places a fertile, spot. The view must indeed be peculiarly grand, embracing a panoramic stretch of the entire bay. The principal mountain in the island, which, as I have observed, is composed of secondary limestone, rises very abruptly to a considerable height above the sea, and has a town at the bottom and another at the top, named Capri and Anacapri, the only communication between which is by means of steps hewn in the rock. About half way up, exists a remarkable grotto, known under the name of "La Grotta del Arco," which presents some singular points of interest to the mineralogist; and near the summit of the island are observed those perfect holes of lithophagous mollusca to which I have already alluded, as bearing in an important manner upon the view I took of the phenomena of the temple of Serapis.

The island of Capri presents an unbroken mass of unstratified Apennine limestone, without any intermixture of rocks of volcanic origin, but the neighbouring shore of Sorrento, which lies between the site of Stabiæ and the promontory of Minerva or La Campanella, displays some facts interesting in the history of the tufas in the Bay. The enchanting territory of Sorrento is a plain of some miles in extent, enclosed by a semicircle of hills on the land side, which are rugged and composed of inclined strata of limestone: these are abruptly cut off by the sea at the two extremities of the plain, which is of a totally different nature, consisting entirely of a compact volcanic tufa, the remains, therefore, of the once extended plain which must have united with the Phlegræan fields on the other side of the Bay, but which, being degraded by the action of the waves, this small portion, defended by a shield of hard limestone on either side from the effects of currents, has remained alone on this side of the Bay to testify its former condition. This tufa, according to Breislak, is more perfect than usual, and appears to abound with trachytic masses inclosing crystalline cavities. It seems quite unnecessary to suppose, with some authors, that this flat semicircular spot was the real bottom of a crater, through which this formation was protruded. It is impossible not to consider it connected with the vast deposit of tufa from which it evidently appears by its situation to have been accidentally dissevered, and that the same causes, whatever they may have been, by which the volcanic district of this coast was formed, had once lined with tufa the eastern flank of the bay upon the substratum of the Apennine limestone. At a subsequent period, perhaps by no sudden phenomenon, but in the slow process of time, the sea itself, which appears to have been so important an agent in the formation of tufa, undermined this friable rock; and if the island of Capri was ever united to the neighbouring headland, their disjunction forming a strait through which a powerful current flows, would be the natural means of washing out the whole of the superincumbent formation, which can have been but feebly united to the underlying rock, excepting only this retiring bay, in which it will be seen from the map it was entirely sheltered from such abrasion by the Capo di Sorrento.

The detritus acquired in this sweep being thrown inwards upon the bay, would defend it from farther degradation, and not improbably the remains of this alluvial deposit might be found in the extensive valley of the Sarno, just where the limestone rocks cease to form the bounding coast.

The true nature of the Sorrentine tufa, which is of some consequence in the investigation of the more recent action of the ocean upon these formations, has, I suspect, been mistaken by Tenore in his *Essai sur la Geographie Physique du Royaume de Naples.* This author divides tufas into primitive and secondary,—a distinction which he thinks may prevent some confusion in the comparison of this rock in different situations; the latter being the "tufo ricomposto," or recomposed tufa of Brocchi, the deposit of particles of original or primitive tufa, carried to a distance, and remoulded by the waters of the ocean. Of this kind Tenore considers the formation in the valley of the Sarno, which corresponds remarkably with the idea I have above thrown out; but the characters of the Sorrentine formation leave no doubt, on my mind at least, that it is an original tufa. Not merely its firmness, which renders it as fit for building as the compact tufa of the other side of the Bay, but its compenetration by masses of trachyte, which cannot be well supposed subsequent to the formation of a friable and recent rock composed of the detritus of previous formations, strengthen this opinion. We are therefore, I conceive, entitled to conclude, that, subsequent to the great retirement of the waters of the ocean, they have effected great changes in the configuration of the Bay, and the disposition of the volcanic products, forming a species of pseudo-volcanic tufa, by the degradation of the original one.

Before explaining the distribution and boundaries of these two very different and very important rocks, it will be proper to return for a few moments to the probable origin of true volcanic tufa,—a subject upon which we touched in the third number of these Notices, and to which we proposed to revert.* But I beg it to be distinctly understood that the observations I am now to throw out, containing a theory in some respects new, I by no means pledge myself to; that I state them as my present ideas on the subject, founded certainly in observations made on the spot, but entirely matured at home; and if they shall be thought suited to awaken the attention of geologists to some new points of view of the subject, my aim will be answered, while I consider myself free to modify or change my opinion as greater experience and more mature reflection may suggest.

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* See this *Journal,* vol. x. p. 251-254.

It was formerly remarked that the submarine origin of tufas seems to be one of the most acknowledged hypothetical deductions in volcanic geology; as Brocchi naïvely enough says, "it must be admitted that here nature has pretty clearly expressed herself, when we find four philosophers at one on the subject."* But though submarine, enough of doubt yet remains to divide the opinions of the learned. Was the tufa ejected from craters?—was it fluid at first, or, falling in dry powdery matter, stratified and agglutinated by the water of the ocean?—or was it not the mere effect of abrasion upon preexistent rocks by the sea, spread in a new form during the period of immersion of our continents?—or in the fashionable theory of the day, was it merely one of the numerous supposed results of the universal deluge? To all these ideas I might perhaps allow some weight, and call in several of them to account for different classes of facts which have often been confounded.† With Tenore and Brocchi, I would distinctly separate the recomposed from the primitive tufas, without, however, attempting to fix the date of the former, which probably may have greatly varied in different situations, and, I would even say, *may* be going on at this moment. Decidedly the most general opinion seems to be, that certain volcanic cones being gradually raised above the sea, the dry materials ejected by them having formed strata under a considerable pressure of water, which likewise put in action the chemical affinity of the particles, which might be of a kind resembling Pozzuolana, and capable of forming a hard rock, the structure of the tufaceous hills gradually rose, and, perhaps, upon coming in contact with the surface of the ocean, received the superposition of those loosely aggregated masses of *rapillo*, pumice and volcanic ashes which appear in the upper beds of these mountains. It may, however, be remarked, that on such an hypothesis the interstratification of such desiccated materials with the indurated tufa (as seen on the Strada Nuova) would be no easier explained than upon other hypotheses. Such generally is the theory of Dolomieu,* Breislak,† Brocchi,‡ Daubeny§ and others, all of whom, probably, took into consideration the other possible modes of origin, and rejected them on what they thought demonstrative evidence. Yet, however applicable their reasoning may appear to certain cases, and especially to the superficial strata, wherever we have an opportunity of more than common insight into the heart of tufaceous formations, the more we shall be disposed to regard it as but a partial interpretation. Had events occurred as there supposed,—had the sea united so intimately with the diffuse matter immersed in it as to induce a cementation of parts, I should infer it to be a *necessary* consequence, that not merely the entire thickness of the rock should be divided into regularly formed strata, such as are generally found near the surface, and these of no great individual thickness,—but that the very structure of the rock should present a stratified conformation. As a striking example, I would ask, how a tufaceous mass such as Ischia, at present 2000 feet above the sea, could be thus produced, without the least appearance of stratification. Supposing it a point of ejection, and that the relative level of the sea was then as much higher than its present one, I would ask, if it can be imagined, within the bounds of probability, that the water of the ocean, if it blended so intimately with the ejected fragments as to agglutinate them, and form its present tufaceous structure, it should not have left decided marks of sedimentary deposition pervading its composition, as well as external arrangement. I am, indeed, aware of the intermixture of tertiary conchiferous clay-beds, but these are local, and impart no character to the general amorphous mass of the island.

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* "Conviene pur dire che la natura assai chiaramente si esprima, perchè quattro filosofi sieno d'accordo." *Suolo di Roma*, p. 195. He alludes to Dolomieu, Santi, Pilla, and Breislak. He might have added his own name and those of many others.

† "It is not improbable," says Dr Daubeny, "that the discrepancy of opinion has arisen from an attempt to extend to all kinds of tuff an explanation suggested by the phenomena of one particular class."—*Volcanos*, p. 411.

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* *Journal de Physique,* tom. xxv.

† *Campanie,* i. 106.

‡ *Suolo di Roma,* 187, &c.

§ *Lectures,* p. 410.

The hypothesis which ascribes the origin of volcanic tufas to mud eruptions has ever appeared one of the most palpably obvious. But since facts, at first sight the most convincing, often yield before maturer inquiry, so in theoretical reasoning, it has sometimes been the practice to seek more far-fetched explanations, where the obvious one, with some modification suggested by phenomena or experience, might have sufficed. One great stumbling-block has, by some of the ablest writers, been considered decisive against the hypothesis of mud eruptions, namely the want of points of origin, and the great thickness of the beds at places very distant from such points. This has been made the *instantia crucis* by Brocchi and Daubeny in particular, and the distance of any existing crater adequate to the effect produced, at Rome on the one hand, and at Andernach on the Rhine on the other, has prevented these observers from considering such an explanation admissible. It is well known that Breislak had upon his creative scheme of craters no such objection to urge; and while he candidly states the coincidence of appearances with such a supposition, especially in the Bay of Naples, he seems to have been hesitatingly withheld by the vague ideas then entertained, as to the existence and extent of mud eruptions in any part of the world, which the researches of Humboldt have now so fully elucidated. Breislak fancied he saw in the seven hills of Rome the walls of imperfect craters,and by merely catching at the general external forms, the volcanic nature of the soil, and with the aid of that fervid imagination which generally accompanies too closely the observer in pursuit of illustrations of a favourite theory, he ended by placing the Forum in the very focus of a volcano. The more correct apprehensions of modern geologists as to the structure requisite to mark out a crater, have dispelled these phantasies, and, as has been eloquently said, "the assertion as to the capitol of the eternal city,—'Capitoli immobile Saxum,'—having been erected on the tottering edge of a crater, however well-suited it may be to point an antithesis, or to illustrate the vanity of human pretensions, rests on too slender grounds to deserve a place in a scientific treatise."* Brocchi, in his work on the Geology of Rome, having confuted the errors of Breislak, proceeds to develope with great elegance, but at considerable length, his own theory, which he supports upon the following chain of reasoning. The submarine origin of the tufa seems indubitable from the number of comparatively recent shells which have been found in different parts of Italy and Sicily; near Rome, at Albano, at Pitigliano in Tuscany, in Ischia, at Pausilipo, at Capo di Monte, and at Iaci, and the Val di Noto, in Sicily.† But if, as the hypothesis assumes, the fragmentary products of conterminous volcanoes formed, by the mere action of the sea this compact rock, it is next to be inquired whence this agglutinative power was derived? Brocchi ascribes it naturally to the properties which we observe in Pozzuolana, and which we have formerly seen to have been so active an absorbent of water, by which a remarkable play of affinities is induced. It is next inquired, whence the erupted matter was derived? The attention is naturally first directed to the obviously volcanic mountains near Albano, as the source of the geological formation of the Campagna di Roma, these being of decidedly igneous origin, and displaying marks of no very remote activity. But in the Alban hills, says Brocchi, we find none of the tufas of the environs of Rome; the Peperino of that vicinity is essentially distinct from any of the rocks which form that vast plain. We must, therefore, look for their origin in a northern direction, where we shall find sufficiently accordant appearances in the mountains of Cimini and the Sabatine lake.

Thus we see the straits to which theorists have been put by the want of craters naturally corresponding to the requisition of a preconceived hypothesis of tufaceous formation;—while Brocchi has thus at Rome been obliged to extend the source of the tufaceous materials to a distance of some twenty-five miles,

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* Daubeny.

† Breislak, in alluding to this fact, asks, may not these shells be derived from the pre-existent strata through which the volcanic matter had passed? But the improvement of mineral conchology has shown that these remains are coëval with, or posterior to the date of the tertiary strata.

Dolomieu got into similar difficulties in Sicily, and Dr Daubeny, from the study of the district of Andernach on the Rhine, was obliged to relinquish the theory of mud eruptions, which he had previously been disposed to consider adequate to the explanation of many phenomena. If it shall not be considered liable to other and stronger objections, the hypothesis which I briefly developed in the third number of these Notices, and which every day's subsequent consideration which I have given to the subject has tended to confirm, will appear more important in its results than at first sight might be obvious. Slight as are its variations from previous theories, if admitted, it proceeds greatly farther towards a rational explanation of facts, and has the farther recommendation of being, to the best of my belief, the result of simple induction from observation, without being carved to suit any more extended chain of reasoning, or either to contradict or support the opinions of any previous writer. In my former sketch I did not develope the origin of the stratified matter which overlies the lithoidal tufa of the vicinity of Naples, and which I would call "secondary tufa," reserving to the mechanically formed secondary tufa of Tenore the name of recomposed tufa, except by referring its deposition to a period anterior to the elevation of the hills of primitive tufa. This will be supplied in the following sketch.

The hills of the Phlegræan fields present us with perhaps a greater variety of structure than some authors have been willing to admit. We have elsewhere seen, that the great mass of the hill of Pausilipo, which, by singular good fortune, we are enabled more deeply to examine than in almost any similar instance, by the aid of the perforation through the heart of it which connects the lateral vallies,—is confessedly devoid of any decisive mark of stratification, * which in my mind is

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* This, I believe, may safely be asserted of Pausilipo, though I think Mr Scrope somewhere speaks of the stratified structure of this original tufa. Some mistakes have, I believe, been founded on the marks left on the upper parts of the sides of the grotto of Pausilipo by the friction of wheel carriages, before the roadway was cut down to its present level. The observation of Spallanzani, which I have mentioned in this *Journal*, vol. x. p. 248, if correct, refers merely to the approximation of vast beds of tufa, presenting no individually stratified structure, but merely a series of *su-*

quite definitive against any idea of aggregation and commixture by an intimate union of divided matter with the water of the ocean. Again, we have true tufaceous craters consisting of superimposed layers, such as the fine example at Miseno; and towards the central ridges of the primitive tufa formation, distinguishing always trachytic craters as of later and independent formation. Respecting the condition of the crateriform summits, either hypothesis stands nearly on a level; but the grand difficulty is, when, as frequently happens, such craters are at a great distance, and even in the Bay of Naples I conceive their agency to have done little towards determining the structure of the ridges of unstratified tufa. But resuming the case of Pausilipo, whence, we may ask, upon the hypothesis of Brocchi, Daubeny, and others, could have arisen the saddle-shaped coating of pumiceous conglomerate which invests the upper part of this and similar hills? It bears indubitable marks of aqueous stratification, yet its position is such that it could not possibly have been thus deposited where it at present appears. For the state of facts is this: The secondary tufa lies on the unstratified rock, *conformably* with the surface of that rock, which, being rounded, it may safely be inferred that it could not have been the effect of subsidence from water; for, however such subsidence may take place, even in the inclined planes of basins, I may safely affirm, that it could not have been so evenly distributed on a convex surface, as we have already shown it to be in describing the sections on the Strada Nuova. The saddle-shape of this superposition likewise forbids the idea that the level of the country was once nearly that of the summit of the ridge, and that the lateral vallies have been the work of subsequent excavation. But a more convincing objection may likewise be made to this. Had the vallies been excavated subsequently to the deposition of the secondary tufa, none of that formation could have been found in those vallies; whereas it appears that the stratified pumiceous rock recommences at the base of the ridge on either side, so that the real section is such as is represented in Fig. 1, where the diagonal shading indicates

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perpositions, such as we might anticipate from successive eruptions from below, as described in the sequel.

the unstratified rock, the horizontal lines the stratified. Such an arrangement can only, I conceive, be satisfactorily explained by one mode of reasoning, namely, that the secondary stratified tufa (or pumiceous conglomerate, or *rapillo*, which are synonymous,) was first deposited under the ocean from craters likewise subaqueous in this neighbourhood, of which traces may or may not remain. That these strata were not agglutinated we have no difficulty in explaining, and have no necessity to recur to the idea generally maintained, that these dry powdery conglomerates were ejected *after* the hills had been raised so high as to emerge from the water. This indeed fails to account for their obviously aqueous stratification. The more obvious reason is, that in their very nature they are unsusceptible of agglutination, being almost wholly composed of filaments of pumice, and, if mixed ever so long with water, must still retain their harsh feel and incoherent structure. We hence derive another argument for the plausibility of our theory, for by all analogy such products are the first ejections of volcanic vents.

When the pumiceous eruption had ceased, a liquid or mud eruption was, I conceive, the next phenomenon, which might either be caused by the admission of the sea water through the craters, or by any other natural agency into which we need not inquire, but is amply supported by analogy. The expansive force of the steam accompanying this igno-aqueous ejection, aided perhaps by the hydrostatic pressure of the fluid mass elevated towards the previous crater, caused, as I imagine, numerous rents radiating from such craters, and through these was slowly ejected a plastic but coherent and amorphous mass of tufa, which, receiving at the same time a sedimentary deposit of rapillo, either actually in suspension in the ocean, or partly derived from the pre-existent strata, was slowly elevated by successive additions from below to its present height, the directions of the hills indicating the lines of fissure, and at their union or crossing forming perhaps the only true tufaceous craters of the neighbourhood. I may add, that by such an explanation alone can I account for the double species of stratification which occurs on the summit of Pausilipo, where some distortion occurring in the regular stratification of the rapillo, and basins of various shapes being thus formed, these are internally filled by perfectly horizontal layers unconformable with the troughs in which they lie. Such must have been deposited after the elevation was complete, and before the final recession of the waters. Until I had brought my ideas on this subject into something of their present shape, I could find no satisfactory explanation of such appearances. Similar considerations may perhaps apply to such a structure occurring in non-volcanic countries, as in beds of coal.

By such reasoning, we throw aside entirely the almost insurmountable difficulties of supposing the origin of the divided volcanic matter to be necessarily from existing, and therefore often very distant, craters, such as at Rome and Andernach. To those who think it necessary to find the existing crater which may have produced every submarine cinereous ejection, I would propose the following question, setting aside the idea of the crater being buried amidst its own subsequent tufaceous formations. The island of Sabrina off the Azores, was formed in 1811 by a submarine explosion of substances, which we may conceive quite similar to those of which rapillo is formed, and which likewise constitute the Monte Nuovo. This island rose 300 feet above the sea, and had a crater 500 feet in diameter.* Very speedily the whole was washed down by the sea, and can be conceived to have become nothing else than beds of secondary tufa. It was below the surface of the ocean in a few weeks; and some years after, its site was covered by *eighty fathoms* of water. Can any reasonable man imagine, that, if the land were elevated to sight, any external traces of a crater would appear?

I will only add one more argument in favour of the liquid ejection of unstratified tufa. Near Capo di Monte, and in other localities near Naples, a very peculiar structure in the tufa is observable, which I have not myself seen, but which has been accurately described. † Certain veins of a decidedly dissimilar structure are observed cutting *vertically* through the mass of tufa. These are considered by Mr Scrope as the mere result of the infiltration of finer particles in the adjoining rock into its fissures. But in my mind, phenomena so similar to dikes in trap rocks and veins in the Monte Somma, already discussed, must be consistently referred to a similar origin. To attribute them to infiltration is bordering upon an approach to the waking dreams of the Wernerian School; and though, from the confessedly semiaqueous origin of the rock, we might look upon it with some toleration in default of more plausible explanation, it appears to me, that it is so entirely what we might expect from a second eruption of tufa from below filling up the fissures of the first, as only to add a strong confirmation to that hypothesis.

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* See this *Journal*, N. S. vol. ii. p. 84.

† Tenore, *Essai, &c.* p. 40; Scrope *on Volcanos*, p. 168; and a paper by the same author in the *Geol. Trans.* N. S. vol. ii.

I have purposely avoided mixing up any account of Mr Scrope's general theory in these remarks; because, though we nearly agree on some points, we greatly differ on others, and on account of the difficulties I have found in arriving at his exact opinion on the subject of volcanic tufa, partly from what I consider the bad arrangement of his book, and partly from the ambiguity, and, as it appears to me, the occasional contradictions of his expressions on the subject. Indeed, it is only latterly that I have been at pains to examine his ideas on the subject, and not until my own were fully formed. The following quotation from Mr Scrope's paper on the Bay of Naples in the *Geological Transactions*, would appear (though it is not without ambiguities,) nearly to coincide in my views. "The hard tufa of which the volcanic hills of the neighbourhood of Naples are almost exclusively formed, seems evidently to owe its coherence, like the trass of the Rhine, to a setting or aggregative process which took place in a body of finely triturated trachyte intimately mixed with water, as that fluid drained off or was squeezed out by superincumbent pressure. In the formations under review this admixture with water appears to have been owing to the circumstance of volcanic rents having burst out under the level of the sea, though in so shallow a spot, that the accumulated ejections soon raised the cones to a certain height above the water level: in consequence of which, the materials subsequently thrown up, falling dry on the surface of the newly raised island, remained in a loose state. Every one of the hills in question, is indeed covered to a greater or less depth by strata of loose tufaceous conglomerate, conformable, and sometimes graduating into the hard tufa below." * In the first part of the extract we are disposed to understand the author as speaking of *mud* eruptions, to which he expressly assigns the origin of these rocks in another place.† Yet the latter part is totally at variance with such a supposition; and, by considering the different state of aggregation, as owing solely to the fragmentary ejections falling above or below water, he merges into the hypothesis of Dolomieu, Brocchi, &c. He is also tied down by the same difficulty which affected these observers,—the want of neighbouring craters in many places; for the hypothesis of protrusion from below, explained above, interferes with the most remarkable peculiarity of Mr Scrope's geological creed, the rejection of the theory of the elevation of horizontal strata round volcanic foci, and of the *erhebungs cratere* of Von Buch,—an hypothesis which it has been seen I consider essential to the explanation of observed facts. Without it I consider Mr Scrope's idea of the origin of the incoherent rapillo as totally untenable; for he imputes its existence to the fact of its being deposited above water, whilst the great consolidated body remained below. How comes it, then, that the rapillo is, as he admits, conformably stratified with the tufa? that, instead of assuming those characters supposed to be owing to the height of the waters of the ocean along a perfectly horizontal line of demarcation, it not only dips on either side of the ridge of Pausilipo to a level much inferior to that which the solid tufa attains at the centre of the hill, but rises with it along its dorsal ridge, as it ascends to meet the lofty crateriform summit of the Camaldoli.—My explanation of the fact is far more universal, and sets out on a matter of observation, which Mr Scrope seems to deny. Speaking of the loose tufa of the Phlegræan fields, in contradistinction to the harder nucleus, he says, that it is "identical in composition with the other, and differing only from it in the incoherence of its parts."‡ Yet he himself admits that the solid tufa is entirely composed of "fragmentary trachyte," while I presume none will deny the characteristic ingredient of rapillo to be fragmentary *pumice.* In this difference I see the explanation of the facts wholly independent of the manner of ejection. The felspathose trachyte disintegrating into clayey matter was easily rendered coherent in the very submarine position, which could have no similar effect on the siliceous particles of pumice. *

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* *Geol. Trans.* N. S. ii. 351.

† *Consid. on Volcanos*, p. 168.

‡ *Ibid.* p. 176.

Mr Scrope appears to me even less happy and more contradictory in his theory of the external arrangement of submarine volcanic formations,—a subject upon which I can here spend only a few words.—We have already seen, that, in the days of Breislak, any appearance of curvilinear continuity in the tufaceous hills of this district was considered ample evidence of a crater. But a growing disposition in geologists to deduce causes from the observed internal structure of the globe soon pointed out that a crater must always be surrounded with strata, of which a vertical section would have indicated corresponding inclined lines on either side of the crater, which produced, would give the section of a perfect cone. For whether these walls of the crater were raised by successive eruptions, or by the elevation of pre-existent strata, they must necessarily assume the virtual form of frusta of cones. Cones formed in the first mode, must necessarily have circular bases from the dispersion of ejected materials being the same in all directions, the quantities of matter and distance from the centre of ejection assuming a proportion dictated by the laws of gravity. Should a series of contiguous craters eject materials, or should these merge into a rent, a ridge will be formed with not a saddle-shaped, but a conical stratification, having an ovoidal base, and the strata *must* be discontiguous at top. By confounding this very important distinction, Mr Scrope, while he has successfully applied his theory to the natural sections of the Capo de Miseno, the island of Nisida, &c.—which, so far from resembling the unstratified tufa of which the ranges of volcanic hills in the neighbourhood are formed, are confessedly and obviously stratified,—would lead the reader into a similar explanation for the great masses of volcanic products which connect these few and insulated craters.* Neither the crater of Miseno or Nisida, (which are the two best defined of this class) could have exerted any material energy on the overpoweringly greater masses connected with them, and they are regular and simple volcanic cones; a section of which, if taken through the axis of the cone, would be a triangle with a re-entrant curve substituted for the apex, and, if the section be taken further from the axis, will be an hyperbola. Now it is from the latter configuration that Mr Scrope would have us to apply his reasoning to a semicylindrical ridge like that of Pausilipo, where any one who chooses to consult a map for its form must see that it is *demonstratively impossible* for such a hill to have been formed in the supposed manner, unless a series of craters had existed all along the top, which the very acknowledgment of the form of the stratification renders impossible. It would thus have had the form shown in Fig. 2, the embouchure above continuing along the top of the hill; or, on the hypothesis of Breislak, only half of this remaining, as in Fig. 3, which is conceived to be the section of one wall of a great crater. How far these are from the reality, may be seen by comparing them with Fig. 1. But perhaps it will be said, that the ridge was deposited in the ejection of solid matter from the crateriform summits near the Camaldoli, which crowns the rising form of the hill: Yet here again we are beset by the insurmountable difficulty of the well-defined form of the ridge rising from the plain on either side, wholly irreconcileable to the idea of being part of a real cone, and depending upon that fact for its form of stratification. But, again, will it be said that this is only a slip of the primitive cone insulated by the action of torrents or other causes. Here the form of the investment of rapillo, upon which Mr Scrope lays the whole force of discrimination of a volcanic mountain, completely interferes, not to mention the unnaturalness of such a dorsal range being preserved, as well as that the existence of pumiceous conglomerate in the adjoining vallies, excludes the idea of the action of torrents. I must, therefore, consider as wholly untenable, the opinion that the eccentricity of the axis of volcanic accumulations produced by one vent, may be indefinitely increased; and in the idea of a chain of craters, the external conformation must be shown to be consistent with such a supposition, which I imagine would not be found either in Pausilipo or the contiguous and similar ridges. I am of opinion that Mr Scrope's theories on this subject require reconsideration and condensation; I have intentionally misrepresented none of his statements, but there is a degree of ambiguity and want of connection about them which renders them difficult to apprehend.

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* I have to notice an oversight in the Sixth Number of these Notices, where I spoke of the Rapillo of the Hill of Pausilipo as identical with Pozzuolana. Now Pozzuolana has characters such as to render its stratification by water almost impossible, since its peculiarity consists in its chemical action with this fluid. It is therefore most likely, as I there suggested, that the real Pozzuolana, such as that of the Bay of Baja, was deposited in dry beds, and never subjected to the direct action of water.

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* *Geological Transactions, ut supra.*

As to the Geological epoch of the Tufas of a nature similar to those of the Bay of Naples, we must judge by their included organic remains. They are for the most part intimately connected with living species. Bivalves of the genus *Venus* have been found at Naples,* and the *Venus Islandica* at Montalto,† *Cardium edule* in Sicily. Near Naples have been enumerated specimens of the genera *Ostrea*, *Cardium*, *Buccinum* and *Patella*, differing in nothing from those at present inhabiting the waters of the Bay.‡ Near Rome bones and grinders of the fossil elephant are found in the tufas as well as in the alluvium.§ Wood is frequently imbedded in this rock, not only at Naples but near Rome and in Iceland; it is monocotyledonous, and Tenore has ventured to put upon some specimens he has seen the name of the *Agave Americana*, which still flourishes in the soil. I need hardly mention a statement of Breislak's, that he had heard of human bones being found in the tufa in the kingdom of Naples at a depth of 76 feet. As the event occurred at a considerable distance of time we must, I am afraid, be content to reckon these among the pseudo-human bones which before the commencement of this century bewildered geologists. For as an Italian geologist forcibly says in similar circumstances, "trista cosa è il dovere ragionare su relazioni di uomini inesperti." All these facts, however, prove that the oldest tufaceous formation was posterior to the deposition of tertiary rocks; in other quarters, however, coeval with them, as their interstratification proves.

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* Tenore.

† Brocchi.

‡ Scrope, *Geol. Trans.* N. S. ii. 350.

§ Brocchi, *Suolo di Roma*, 179. I have in my possession a portion of a tusk dug from the alluvial bed of the Anio when I was there in 1827.

What I have called primitive tufa chiefly exists in the vicinity of Naples. The great plains which appear to have owed their origin to the volcanos of that hilly district, extend to a great distance, and are probably but fragments of more extensive deposits. The Ponza islands appear to be remnants of a stretch of similar formations, which may have one day united with those of Albano and Rome, and to the origin of which some intermediate vents, as those near Sessa, the ancient Suessa Auruncorum, and Velletri may have contributed.

Succeeding the tufas in date may probably be reckoned the trachytic rocks; of these we have some notable examples in Ischia, in the Phlegræan fields, and at Sorrento. It has been well observed by Mr Scrope, that, as a volcanic vent, Ischia is contrasted with Vesuvius by its trachytic character, and in all probability it had much the earliest date, though we can infer nothing from historical records, since, notwithstanding the ample evidence we have of the violent commotions of the Ischian volcano at a period long preceding that of the first eruption of Vesuvius within the memory of man, the latter we know must have owed its existence to long anterior action, and actually exhibited external marks of its origin before the eruption under Titus. This accounts for the very distinct aspects of these two volcanic vents embracing the region which we have attempted to describe. In the intermediate space several intimations of similar protrusions appear, which, as far as we can judge from external appearances, were probably ejected like the trap rocks through the superincumbent mass of tufa. Of this description is the *Piperno* of the hill of the Camaldoli.* It might be worthy of inquiry whether this elevatory action of vast masses of trachyte was not the cause of the emersion of the tufaceous formations from their submarine condition.

The trachytic series is altogether extremely varied and complex in the Bay of Naples, and different varieties of it may probably belong to very different epochs. We have it purely felspathose, or including augite, or quartz, compact, porphyritic, vesicular, or conglomerated as in the Monte di Procida. Volcanic clinkstone, which occasionally occurs, may also probably be included under the trachytic class; such we have mentioned as occurring in the Monte Nuovo and in Ischia.

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* See this *Journal*, x. 254; and see Note E.

There are many transitions from the real trachyte to modern lavas, and these we naturally find in spots which indicate an intermediate period of activity, such as Astroni and the Solfatara, especially the latter. The Monte Olibano descending from the crater of the Solfatara to the sea, which has already been described, partakes remarkably of this intermediate character.

As they must necessarily depend greatly upon the fundamental formations, the characters of modern lavas are different according to the spots from which they issue. The *coulées* of the island of Ischia are more refractory than those of Vesuvius, and the characters of the latter are essentially the same as the ancient products of the same source of which the houses in Pompeii were built, before being overwhelmed by the first eruption which history records. The original lavas, too, of the Monte Somma demand attention, and by their remarkable configuration afford hints for the interpretation of other natural appearances. Finally, the frequent eruptions of Vesuvius, if their number renders them often less imposing, that very moderation contributes to their value as scientific lessons. It opens an ample field to the natural philosopher, the geologist and the chemist; and if, as we may hope, the problem of volcanic action be one day solved, in all likelihood Mount Vesuvius will be the most fertile source of information. The researches of Davy have opened the way in this career by a series of well conducted and really analytic experiments, and, it may safely be affirmed, amidst the daily increasing perplexities which accompany the progress of geological inquiry, that the action of heat and its effects upon the mineral formations is one of the most important objects of investigation, as well as most reducible to inductive research,—*important*, because the extent of its influence is daily more acknowledged,—

and *sure*, because we may compare ancient results with still existing causes of formation, which cannot be said of any others of the great rocky strata of our globe.

It was once my intention to have enlarged these Notices with some details of other subjects connected with physical geography, particularly as to climate. But the strictly topographical details which have, more than I proposed, filled these pages, would render any synopsis of other physical topics which could come within the limits I proposed to myself, but an imperfect and unconsolidated appendage. I shall therefore content myself with inserting a few facts in a Note,* among those which I shall now subjoin in illustration of the preceding series of papers.

NOTES ON THE PRECEDING PAPERS ON THE BAY OF NAPLES.

NOTE A. (No. I. *Vesuvius.*) Vol. ix. p. 193. *On the size of the crater before the eruption of A. D.* 79.

The size of a circle, of which the present ridge of the Monte Somma forms a part, would not have been so enormous as I once supposed, though sufficiently large to leave my opinions on the hypothesis here referred to unchanged. From an accurate projection upon Breislak's large map of Vesuvius, I find the radius of curvature of the ridge of the Somma to be 1$\frac{4}{10}$ Italian or geographical miles, and the arc subsisting 120.° Still the area of the crater must then have been *nine times* greater than at present, yet it is now perhaps the largest in existence. On the subject of the dikes in the abrupt face of Monte Somma, I may refer to the following works: Sir James Hall on the Strata near Granite, *Edinburgh Transactions*, vol. vii. Necker de Saussure, *Mémoires de la Societé Naturelle de Geneve*, tom. ii.† and *Geological Transactions*, vol. iii.

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* See Note L.

† It reflects no honour on the pains taken in the public libraries of Edinburgh to preserve complete sets of Transactions which few individuals possess, that this volume, containing M. de Saussure's Views on Tufa, which I had also wished to see, is not to be found in the Libraries of the University, the Faculty of Advocates, or the Royal Society.

NOTE B. Vol. ix. p. 206, and vol. x. p. 136. *On the volcanic sand of Vesuvius.*

The chemical composition of this finely divided matter is extremely complex, and likewise very variable; hence it has of late years occupied a good deal of attention. The assertion, that gold has been detected as a component part, I took from an Italian work, and mentioned in the first of the above cited passages, but having found an express contradiction of it in Humboldt's *Tableaux de la Nature*, upon such authority as Monticelli and Covelli, and Rose, I took the earliest opportunity of correcting it in the succeeding number. Since that period, however, a more detailed account of the matter has been given by Lavini, an Italian chemist, in the *Turin Transactions*, * who has cited a sufficient number of analyses to show, that not only the materials of the volcanic dust of different eruptions greatly vary, but also of the same one. As his results are curious, and also throw light upon the occurrence of native metals in this form, I shall here cite them.

Eruption of 1794. *Lavini.*	
Bituminous vapour of water,	2.15
Sulphate of lime, -	2.00
Muriate of Soda, -	1.00
Lime, - -	2.00
Oxide of copper, -	10.00
Alumina, - - -	3.15
Tritoxide of Iron, -	9.00
Magnesia, - -	2.00
Silica, - -	68.00
Loss, - -	0.70
	100

Eruption of 1822. *Lavini.*	
Water, - -	3.13
Muriatic acid, -	
Muriate of ammonia, -	
Sulphate of Lime, -	6.50
Muriate of Soda, -	1.50
Lime, - - -	2.07
Oxide of Iron, -	13.50
Alumina, - -	15.00
Magnesia, - - -	1.50
Silica, - -	53.50
Carbon, - - -	2.10
Loss - - -	1.20
	100

The following lists contain a view of the constituents found by different chemists in the volcanic sand of 1822, which present so much diversity as to render us scrupulous about rejecting the assertions of any one observer as to the traces of simple substances which he may have detected.

Vauquelin.—1. Silica; 2. alumina; 3. oxide of iron; 4. muriate of ammonia ; 5. sulphate of lime ; 6. sulphate of potassa ; 7. copper ; 8. manganese ; 9. carbon ; 10 lime.

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* *Memorie dell' Academia reale della Scienze di Torino,* tom. xxxiii.

Lancelotti.—1. Sulphate of lime ; 2. muriate of ammonia ; 3. muriate of soda ; 4. sulphate of soda ; 5. sulphate of alumina ; 6. vegeto-animal matter ; 7. a trace of ammoniacal salts ; 8. subcarbonate of peroxide of iron ; 9. alumina ; 10. silica.

Pepe.—1. Sulphate of potassa ; 2. sulphate of soda ; 3. subsulphate of alumina ; 4. subsulphate of lime ; 5. subsulphate of magnesia ; 6. muriate of potassa ; 7. muriate of soda ; 8. alumina ; 9. lime ; 10. silica ; 11. magnesia ; 12. tritoxide of iron ; 13. antimony ; 14. trace of gold ; 15. trace of silver.

Lavini.—1. Sulphate of lime ; 2. muriate of soda : 3. lime ; 4. oxide of iron ; 5. alumina ; 6. magnesia ; 7. silica ; 8. carbon ; 9. water ; 10. muriatic acid ; 11. muriate of ammonia.

NOTE C. Vol. ix. p. 212. *Vesuvian Minerals.*

I am not going to attempt to improve this extremely imperfect paper. But it may not be altogether without interest to enumerate the principal new minerals discovered by Monticelli and Covelli, and described by them in their *Prodromo della Mineralogia Vesuviana,* which have not found their way, I believe, into any Scottish Journal at least.*

Humboldtilite. Primitive form, a rectangular prism with a square base. Sp. gr. 3.104. Scratches glass. Colour, brown. Lustre vitreous. Translucent.

Zurlite. A variety of the last. Sp. gr. 2.274. Scratched by the knife. Asparagus coloured. Lamellar.

Davina or Davyne. A variety of nepheline. Primitive form, hexahedron. Colour, brown, white. Lamellar. Usually occurs in prisms of various forms. Sp. gr. 2.25. See this *Journal,* No xiv. p. 326, and No. ii. N. S. p. 368.

Cavolinite, nearly resembles Davyne.

Cristianite. Primitive form, oblique rectangular prism. Sp. gr. 2.77. Scratched by quartz. Colour, brown, yellow, reddish.

Biotina. Somewhat similar to the last. Scratches glass. Lustre vivid. Sp. gr. 3.11.

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* These notices are from Silliman's *Journal,* Oct. 1826.

Foliaceous copper. Very delicate sublimations of copper.

Breislakite. In appearance a brownish or reddish-brown down. Seen by the microscope in acicular crystals. It is found in the lava of La Scalla, at Olebano and Pozzuoli. It consists of silica, alumina, and a little iron.

I shall make no farther additions to this paper, the subject of which would alone require volumes. I must take this opportunity of acknowledging the compliment which that excellent mineralogist, Professor Leonhard, has paid me, by translating this and the succeeding notices into German in his *Zeitschrift für Mineralogie.* The only one which I have seen is a neat abridgement of No. iii. On Pausilipo, &c.

NOTE D. (No. II. *On the Buried Cities,*) Vol. x. p. 120. *On the Silence of Pliny respecting the fate of Pompeii.*

In the *Bibliotheque Universelle* for 1829, where the greater part of this paper has been translated, I find the following annotation. " Ce silence est, ce me semble, facile à expliquer. Pline ecrivoit à son ami Tacite qui lui avoit demandé la relation de la mort de son oncle ; il ne devroit donc entrer dans les details qui se n'atachoient à cet evenement particulier ou à la nature du phenomene. L'ensevelissement d'Herculaneum et de Pompeii n'etoit certainement pas ignoré de Tacite, et Pline n'avoit qui faire de lui en parler." (R.)

NOTE E. (No. III. *On the District of Pausilipo, &c.*) Vol. x. p. 254. *On the rock called Piperno.*

This species of trachyte, for such it must be considered, is sufficiently characteristically described in Mr Scrope's memoir in the *Geological Transactions,* which has been published subsequently to the article before us. Since writing it I have also had occasion to remark a structure very similar to that here described in the felspathose trap rocks of the eastern Pentland Hills, near Edinburgh, where the augite displays a similar concretionary separation. It appears both in patches and cavities in the felspar basis, having an elongated form exactly as described in the case before us, and presents so much of a fused appearance, as might readily give Breislak the idea of its being pitchstone. This striking similarity will give some assistance in determining the true geological place of this rock, and will likewise give us reason to suppose, as was hinted at in the text, that, like the trap rocks, it was protruded through the superincumbent mass of tufa.

NOTE F. (No. IV. *On the Solfatara.*) Vol. i. N. S. p. 133. *On the connection of the Solfatara with Vesuvius.*

On this point, and on the interesting question of the subterranean cavity before the area of the Solfatara, on both of which I have in this paper strongly expressed my opinion, I have the satisfaction of being able to cite the authority of Sir Humphry Davy, a philosopher whose sober judgment so admirably tempered his ingenious sagacity, as to render even his hypothetical deductions highly valuable. He observes: "There is no question but that the ground under the Solfaterra is hollow, and there is scarcely any reason to doubt of a subterranean communication between this crater and that of Vesuvius. Whenever Vesuvius is in an active state, the Solfaterra is comparatively tranquil. I examined the bocca of the Solfaterra on the 21st of February 1820, two days before the eruption of Vesuvius was at its height. The columns of steam, which usually arise in large quantities when Vesuvius is tranquil, were now scarcely visible; and a piece of paper thrown into the aperture did not rise again, so that there was every reason to suppose the existence of a descending current of air.* The subterraneous thunder heard at such great distances, under Vesuvius, is almost a demonstration of the existence of great cavities below filled with aeriform matter, and the same excavations which in the active state of the volcano throw out during so great a length of time immense volumes of steam, must, there is every reason to believe, in its quiet state, become filled with atmospheric air.†"

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* In 1814, in 1815, and in Jan. 1819, when Vesuvius was comparatively tranquil, I observed the Solfaterra in a very active state, throwing up large quantities of steam and some sulphuretted hydrogen.—Davy.

† Sir Humphry Davy on the Phenomena of Volcanos, *Phil. Trans.* 1828. Part. i.

NOTE G. (No. V. *On the Temple of Serapis.)* Vol. i. N. S. p. 275. *On the Theory of De Jorio.*

Some time since, I had, through the kindness of Dr Hibbert, an opportunity of consulting De Jorio's work *on the Temple of Serapis*, which, at the time of writing this paper, I had failed of seeing by any means. I happened at the time the work was in my hands to be so engrossed by other objects, that I had not leisure to do more than glance over it. I saw enough, however, to be made aware of a fact to me most unexpected, that this author does not support the lacustrine hypothesis, which, in the paper to which this note refers, I had taken considerable pains to refute. I had conjectured that De Jorio was of this opinion, (p. 295, *note*,) because, in his work upon Pozzuoli, he speaks in general terms of the pholades inhabiting a "laghetto" or small lake, and because Dr Daubeny, in quoting the author, expressly infers him to support the idea of the lake being separated from the sea, and at a higher level.* But, from what I saw of De Jorio's work, I believe I am correct in asserting, that his "laghetto" was nothing but a small arm of the sea, which, communicating with it, retained the same level; and he therefore supposes a real change in the relative level of the Temple and the sea exactly as I have done. Indeed, it give me very unexpected pleasure to observe, that, while I thought I was going directly against the Canonico De Jorio, we seem (with the exception of his supposition that the temple was *built* under high water mark,) to have been pursuing almost the same course, derived from similar data; a circumstance highly confirmatory of any hypothesis supported without collusion.

NOTE H. (No. VI. *On the District of the Bay of Baja.)* Vol. ii. N. S. p. 77. *On the formation of the Monte Nuovo, &c.*

It is sometimes not uninstructive to notice the gross mistakes which, at no very remote period, might be made by respectable and even distinguished travellers without instant detection,—a lesson more peculiarly to be impressed upon those who visit less explored countries, with an imperfect acquaintance, or total ignorance, of the languages and customs of the natives, but which might with advantage be attended to by the multitude of superficial travellers who at present overrun Europe, and encumber the press with their ill-digested lucubrations. I chanced lately to look into the travels of no less a man than Bishop Burnet, where I found within a few pages such a mass of errors, as might well have been distributed through the whole work. The following singular fancy respecting the origin of the Monte Nuovo is sufficiently striking;—"The Sulfatara is a surprizing thing; here is a bottom out of which the force of the fire that breaks out still in many places in a thick steaming smoke that is full of brimstone, did throw up about a hundred and fifty years ago, *a vast quantity of earth, which was carried above three miles hence, and formed the hill called Monte Novo.*"* It is rather extraordinary to hear such an account at a distance of time from the event,—only half of that which has now elapsed. The Bishop tells us that he paced the Grotto of Pausilipo, and found it 440 paces. I found it to be 777 yards by pacing, which differs only three from the measured length reduced to English feet, which is 2322; Bishop Burnet must therefore, by his own account, have stepped out more than *five feet.* I will not quote any more of the errors which may be found within a page or two of those just cited. They manifest a want not only of natural history and of ordinary observation, but of classical information. He is, however, accurate upon one point on which the writers of the last century were extremely ignorant,—the depth of Lake Avernus, which, it is rather remarkable, should, at so early a period, have been accurately fathomed, and yet, till within a few years, have retained its mysterious character. Bishop Burnet states its depth at 18 fathoms or 108 feet; and we have seen (No. vi. p. 86,) that Captain Smith has determined it to be 102 feet.

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* *Volcanos*, pp. 163, 164.

NOTE I. (No. VII. *On the Islands of Procida and Ischia.*) Vol. ii. N. S. p. 342. *On the origin of Serpentine.*

The remarks which in this paper the very curious fact of the occurrence of serpentine in Ischia induced me to throw out on the origin of that remarkable rock, were rather intended to direct the attention of geologists to some singular coincidences which its occurrence in this volcanic spot presented, than to hold up as a fact, that the particular specimens in question owed their *formation* as well as their *position* to volcanic action. The facts connected with its geognostical situation were indeed so curious as to induce me to examine the authorities for and against the igneous origin of the rock; but its remarkably fine *mineralogical* character seeming to point to a primitive, prevented me from assigning to it with any confidence a volcanic formation, though I am much disposed to believe that it has passed through the volcanic focus, and owes its present situation to that agency. In fact, after bringing together the authorities for the igneous production of the rock in general, an opinion which I am at present disposed to entertain, I added, as to the immediate instance in question; "but even if we should not be disposed to admit its presence as an indigenous rock (meaning in this case ignigenous,) it may at least have been elevated by volcanic explosion from the deep-seated bases of the Apennines, which, in different parts, as Lombardy, Tuscany, Calabria, and Sicily, display this rock in remarkable perfection."—Pp. 343, 344.

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* *Letters on Switzerland, Italy, &c.* written in 1685, and 1686. Amsterdam Edit. 1687, p. 216.

I quoted in page 342, a remarkable instance of the volcanic occurrence of serpentine, said to be recorded by Sir George Mackenzie in his account of Iceland. Since writing this paper, Dr Brewster has communicated to me a letter from Sir George Mackenzie, pointing out the mistake into which I had fallen, and stating that his *Travels in Iceland* contained no account of serpentine in the mountain of Akkrefell. I greatly regret this mistaken report of Sir George's observations, as I now find it to be, but, at the same time, I fear that the source in which the error originated must have done much more harm than any inadvertence of mine could produce. I took the statement from a paper of Brongniart's, on the serpentines of Italy, translated in M. de la Beche's volume of *Foreign Geological Memoirs*. In this work the passage runs thus:—"Sir G. Mackenzie states, that the volcanic amygdaloid beds of the mountain of Akkrefell in Iceland are traversed by veins of serpentine of more than a metre (about 3 feet 3½ inches,) in thickness." After having the mistake pointed out to me, I was anxious to investigate its source, and referred to the original paper of M. Brongniart in the *Annales des Mines*. To show that the translator was not in fault, I shall quote the words of the distinguished French geologist himself:—" M. Mackensie dit, que les couches d'amygdaloïdes volcaniques de la montagne d'Akkrefell en Islande sont traversées par des veines de serpentine de plus d'un metre de puissance.*" Though I generally make it a rule to consult every work I quote by name, which is accessible to me, I shall not in this case be thought guilty of undue inattention, when, besides the accidental circumstance of being much hurried in the completion of this paper, I made use of such high authority as that of Brongniart.

NOTE K. Vol. II. N. S. p. 346.—*On the Mineral Waters of Ischia.*

The numbers which I have here given as a rude approximation to the contents of the Gurgitello hot spring, and which were deduced from the description of Andria, seem to be either incorrect, or applicable to some other of the numerous neighbouring springs; for in Dr Clark's work *on Climate*,† I find the following analysis by Lancelloti, professor of chemistry at Naples:—

Contents of a pound. Spec. gr. 1.0065.

Free carbonic acid, - -	2.195 grains.
Sulphate of soda, - -	3.549
Sulphate of lime, - -	0.375
Muriate of soda, - -	15.425
Carbonate of soda, - -	13.631
Carbonate of lime, magnesia, and iron,	0.500
Silica, - - -	0.375
	36.050

Analysis of the saline efflorescences on the walls of the Stufe, mentioned p. 346. By Dr Daubeny.

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* *Annales des Mines*, 1821, p. 197, *note*.

† Page 172.

Sulphate of soda, - - - - -	51.0
Muriate of soda, - - - - -	2.3
Carbonate of lime, - - - - -	5.2
Silica and other matter, insoluble in water and in acids,	3.6
Water and loss, - - - - -	37.9
	100.0

NOTE L. *Notice respecting the Climate of Naples.* See this number, page 250.

In promising a note on this subject, I merely proposed that it should contain a very few *facts* on this important, but little investigated subject. Where data are awanting, I shall not spend time by swelling this already too long paper by detailing the mere common places which supply their want. While the climate of Naples is a theme of general admiration, and its perfections have perhaps been exaggerated as a winter residence, few have thought it worth while to analyze its qualities, and Tenore, in his work on the Physical Geography of the Kingdom, has been obliged to confess the meagreness of his materials for the city itself, and their almost total want in the extensive districts of Calabria, Abbruzzo, and La Puglia. The indefatigable Humboldt, in his work on Isothermal Lines, has been obliged to omit this important station in his list of ascertained mean temperatures; and the only good observations have been made since the time of the publication of that work, by Broschi, the astronomer-royal at Capo di Monte. His observations have been reported and reduced by Tenore, and by Dr Clark, in his excellent meteorological tables appended to his work *on Climate.* As they are reduced to English measure we take them from the latter.

The position of the point of observation close to the town of Naples is in north latitude 40°, 51′, 10″; longitude east of Paris in time 47′, 48″, and about 240 feet * above the sea. The period is from 1821-1825; and the hours of observation are sunrise and 2 P. M. which in this climate we should not consider very satisfactory for giving the mean temperature;

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* Or 74 metres, as given by Tenore; Dr Clark makes it 148 metres or exactly double. But I have selected the former as the most probable in itself, and also the most likely to be correct.

but as we find it 61°.40, or almost what we should expect from the analogy of Rome, we may conceive that the accuracy consists in a compensation of errors; the temperature at sunrise being a little above the minimum; that at 2 P. M. a little below the maximum. In fact, by examining the admirable hourly observations conducted at Leith, we find the defect of the mean annual temperature at sunrise below the mean = 3°.16; the excess at 2 P. M. = 3°.04.

Tabular View of the Climate of Naples.

	Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Mean temp	61.40	46.5	48.5	52.0	57.0	66.5	71.0	75.0	76.5	72.5	65.0	54.5	50.5
Maxima,	93	58	60	69	78	86	88	93	91	88	79	64	61
Minima,	29	29	31	38	43	51	56	64	62	60	51	44	31
Range,*	64	29	29	31	35	35	32	29	29	28	28	20	27
Range of Bar,† in Eng. in.	1.154	0.888	0.843	0.888	0.710	0.355	0.552	0.266	0.365	0.488	0.532	0.621	0.621
Rain in Eng.in.‡	29.256	3.472	1.221	4.631	2.315	0.925	2.126	0.614	0.748	2.146	4.354	2.980	3.724
No. of fine days.	210	17	17	13	17	18	17	29	16	17	17	18	15
Cloudy,	58	3	5	1	6	7	4	1	3	5	3	6	5
Rainy,	97	11	6	10	8	6	9	1	12	8	11	6	11

* This range not being derived from register thermometers, comprehends only the limit of observations.

† Mean height for the year 29.554 inches.

‡ These numbers not being given by Dr Clark, are reduced from those in centimetres recorded by Tenore.

From the situation of the town and Bay of Naples, freely exposed to the sea on one side, with a hot sun directed upon a slope laid out to its meridian intensity, and on the other not distant from the lofty summits of the Apennines, these spots are exposed to great and sudden changes of temperature, as the indication of ranges in the preceding table sufficiently shows. Hence also it is peculiarly at the mercy of the winds, each of which have their peculiar character. The north wind (Tramontana) is rather prevalent in winter, and is cool, as also the north east and east, (Greco and Levante,) which blow with considerable violence from the Apennine range. The south east, or Scirocco, is not unfrequent at all seasons; and in my opinion is, at least during winter and spring, not less oppressive than that so much complained of at Rome. It blows sometimes even with violence, yet still preserves its depressing and unrefreshing character, high temperature, and dampness: it is said sometimes to convey the sand of the African deserts to the shores of Italy. The Libeccio or south west wind, blowing the whole western mass of the Mediterranean towards the mouth of the Bay, and coming in violent squalls, raises in a few minutes the most violent tempests, the rapidity seeming the work of magic, and rendering the navigation of the Bay frequently dangerous, as those who have had any experience in its treacherous surface must be aware. Spring is the least defined of the Neapolitan seasons; but this very fact is the principal boast of the climate, for of few others can it be correctly said.

"Hic ver assiduum, atque alienis mensibus æstas."

The mild winter, insufficient to stop the course of vegetable growth, rushes into the splendour of the Italian summer. Intolerable as is the heat of the sun at the latter period, when unaccompained with fresh breezes, the mild winds of the west predominate so much, as to render the climate far from insupportable, and on the coolest shores of the Bay, as at Ischia and Sorrento, nothing but delightful. At Naples itself the ordinary temperature of the height of summer is from 77° to 84°, hardly ever rising to the extremes given in the table, and never continuing at them. The winter is mild, but rainy, especially in November and December. I have given in this *Journal* for July 1828, a meteorological register for part of those months, showing the unsettled weather which characterizes them. After Christmas there is generally some cold weather, during which Vesuvius and the Apennines get a coating of snow, which, however, rarely falls, and hardly ever lies a day in the capital. The thermometer has been known to fall to 23° Fahr.

This sketch is all which space permits me at present to offer. For some account of the botanical geography of the kingdom of Naples, I may refer to the work of Tenore. The state of agriculture, the prolific nature of the *Terra di Lavoro*, the mode of growth of the palm, olive, and vine in this district, are subjects little attended to in this country, and would require considerable elucidation.*

* Whilst this sheet is passing through the press, I have seen with great satisfaction Mr Lyell's excellent work on Geology, and find with pleasure

ART. IX.—*An Account of Experiments to determine the quantity of Light reflected by Plane Metallic Specula under different Angles of Incidence. With a Description of the Photometer made use of.* By R. POTTER, Esq. Junior, Communicated by the Author.

HAVING followed the grinding and polishing of lenses and specula for many years as my favourite pursuit in leisure hours, by frequent practice in the operative part, and many experiments to improve the polishing powders, I began about eighteen months ago to attain some proficiency in this difficult art, and wished to know how near my specula approached to those of the late Sir William Herschel; who, in the *Phil. Trans.* for 1800, has given measurements of the quantities of light reflected by the plane specula which he used in his telescopes.

These measurements he took on a plan similar to that of Bouguer, which he has described in his "*Traité d'Optique*," but this work I was not able to meet with, and not being then aware that Priestley in his *History of Vision* has described the instruments used by him, I was left to make such modifications of Count Rumford's plan of comparing the intensity of shadows as promised to make it serviceable in experiments where considerable exactness was required, and the speculum under examination but of small size. To avoid the stronger illumination which surrounded the shadows, and to find some description of light or lamp which was not subject to sudden variation, were the most essential objects, and the various mechanical contrivances detailed in the description, at the end of the paper, were adopted from time to time, as found necessary, to enable me to go through the measurements in less time and with more certainty. The plan of this photometer I find is in principle the same as that of one of Bouguer's; but, as given by Priestley, his instrument was in a state much too incomplete for inquiries similar to those which are the subject of the present paper; and there still remain sources of uncertainty and inaccuracy which it is difficult to surmount. The one arises

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that he coincides in almost all my views developed in these papers, as far as they were published before his work. His opinions on the Temple of Serapis in particular are strictly the same as my own, and I am glad to observe that he has corroborated them by some new analogies.

from the fatigue of the eye experienced by looking long and intently at bright objects surrounded by darkness, which prevents it after some time judging accurately of very small differences, but this may in a great measure be surmounted by frequent practice. The variation in a few minutes of the quantity of light given out by every lamp, &c., which I have yet tried, is a still greater source of uncertainty, and to which, I believe, may be attributed most of the irregularity visible in the results about to be given.

The principle of the measurements by the photometer is this, that the light incident on a given surface varies inversely as the square of its distance from the luminous object. Of the two lamps which are used, the one being for the purpose of comparison, remains stationary during the measurements, while the direct light of the other lamp, or the reflected light from the speculum, are made to give an equal illumination with it on the thin paper in the screen; and the quantity of light reflected is found by comparing the squares of the distances which it has passed over to give the equal illuminations.

In this way the direct light at 40 inches is equalled only by the same light received through the medium of a plane reflector of speculum metal when at 32 to 33 inches from the screen, or out of every 100 rays received by the reflector only about 64 to 69 arrive at the screen.

A small oval speculum of about 1½ inch in length, and 1 inch in breadth, and composed of about 14½ parts of tin to 32 of copper, and highly polished, being tried in this manner, gave 65.53 rays of every 100, as reflected at 18° incidence, which is about 1.73 less than the average given by Sir William Herschel, who does not state the incidence at which he took them, but only that it was nearly perpendicular. It was the opinion of Sir Isaac Newton, (see his Letter in *Phil. Trans.*, dated Cambridge, May 4th, 1672,) that metallic specula, in common with all other substances, reflected light "most copiously, when incident most obliquely," and this opinion has been adopted by every writer on the subject since. Priestley, after giving the results of some of Bouguer's experiments with black marble and other substances, says, "similar experiments made with metallic mirrors always gave differences much less considerable. The greatest was hardly ever an eighth or ninth part of it, *but they were always the same way*," that is, he found more light to be reflected by the mirrors which he used when incident obliquely than when incident perpendicularly. When I tried the above speculum with the angle of 45° incidence, the average of 18 measurements, viz. 12 direct and 6 reflected, gave only 64.90, as the quantity reflected, showing a loss, instead of a gain, as the angle of incidence was increased; but the apparent difference between 18°, the incidence at which the former were taken, and 45°, was so small that I thought it possibly might arise from accidental inaccuracies; but another set taken at 66° to 70° incidence, giving a still less quantity, I was convinced the fact was just the contrary to the general opinion in *highly polished* metallic surfaces. I knew this loss could not happen from the speculum not being truly plane, for, from the way in which the polish had proceeded, it must, if any thing different from plane, be slightly concave.

These experiments involving a very important question not only in the science of optics, but also in the general relations of ponderable matter and light, I determined to bring the photometer into as convenient a form as possible, and to try them again with every precaution to insure as correct results as the nature of the subject would admit. Accordingly, on the 8th January last, having got a small oval speculum of the same size and composition as the one before-mentioned, to a very fine and high polish, and having used a new contrivance in the grinding and polishing, which made me certain that it was very nearly a true plane, I tried it in the photometer, and found, as I had formerly done, that less light was reflected as the angle of incidence was increased. Having in the meantime made some further alterations in the photometer on the 25th January, I obtained the following measurements:

Angle of incidence.	Average of trials.	Light direct.	Light reflected.
20°	3 each,	$36\frac{11}{12}+1\frac{1}{5}=38\frac{7}{60}$	equalled $27\frac{5}{6}+3\frac{14}{15}=31\frac{23}{30}$ in.
40°	3	$38\frac{1}{12}+2\frac{11}{20}=40\frac{19}{30}$	$29\frac{1}{2}+3\frac{17}{24}=33\frac{5}{24}$
60°	3	$38\frac{1}{3}+3\frac{7}{10}=42\frac{1}{30}$	$29\frac{7}{12}+4\frac{17}{60}=33\frac{13}{15}$

and

20° incidence $(38\frac{7}{60})^2 : (31\frac{23}{30})^2 :: 100 : 69.45$ rays reflected.

40° $(40\frac{19}{30})^2 : (33\frac{5}{24})^2 :: 100 : 66.79$

60° $(42\frac{1}{30})^2 : (33\frac{13}{15})^2 :: 100 : 64.91$

I take the measurements alternately, first one of the direct, and then one of the reflected light, until I have a sufficient number of pretty uniform distances, rejecting them and commencing again when there has been any considerable variation in the lights. In this way the above did not vary more than $\frac{1}{4}$ of an inch in any two neighbouring trials of the same description.

The following I obtained on the evening after.

Angle of incidence.	Average of trials.	Light direct.		Light reflected.
10°	3 each,	$39\frac{1}{6}+2=41\frac{1}{6}$	equalled	$26\frac{5}{6}+7\frac{4}{15}=34\frac{1}{10}$ in.
30°	3	$38+1\frac{19}{20}=39\frac{19}{20}$		$28\frac{11}{12}+3\frac{15}{22}=32\frac{79}{132}$
50°	3	$38\frac{1}{3}+3\frac{1}{12}=41\frac{5}{12}$		$29\frac{7}{12}+3\frac{11}{12}=33\frac{1}{2}$
70°	3	$38\frac{1}{8}+4\frac{8}{12}=42\frac{19}{24}$		$29\frac{1}{2}+5\frac{1}{24}=34\frac{13}{24}$

and

10° incidence $(41\frac{1}{6})^2 : (34\frac{1}{10})^2 :: 100 : 68.61$ rays reflected.

30° $(39\frac{19}{20})^2 : (32\frac{79}{132})^2 :: 100 : 66.58$

50° $(41\frac{5}{12})^2 : (33\frac{1}{2})^2 :: 100 : 65.42$

70° $(42\frac{19}{24})^2 : (34\frac{13}{24})^2 :: 100 : 65.15$

Three other trials taken at 10° incidence, but in which the lights had varied too much, gave 70.32 as reflected, and 3 in the same way at 70°, gave 65.91. Though the results of this set do not well agree with those of the former, yet there is clearly a convergency in the differences, which must be allowed in spite of the uncertainty which attends this sort of experiments; where, if there were no other reason, the difficulty of the eye judging of such small differences would cause apparent irregularities. On the 27th January I obtained the following:

Angle of incidence.	Average of trials.	Light direct.		Light reflected.
10°	3 each,	$38+2=40$	equalled	$25\frac{1}{3}+7\frac{4}{15}=32\frac{3}{5}$ in.
30°	3	$37\frac{7}{12}+1\frac{19}{20}=39\frac{8}{15}$		$28\frac{5}{16}+3\frac{15}{22}=31\frac{175}{176}$
50°	3	$37\frac{23}{24}+3\frac{1}{12}=41\frac{1}{24}$		$29\frac{5}{48}+3\frac{11}{12}=33\frac{1}{48}$

and

10° incidence $(40)^2 : (32\frac{3}{5})^2 :: 100 : 66.42$ rays reflected.

30° $(39\frac{8}{15})^2 : (31\frac{175}{176})^2 :: 100 : 65.50$

50° $(41\frac{1}{24})^2 : (33\frac{1}{48})^2 :: 100 : 64.73$

A set of 3 trials taken at 70°, which were very irregular, gave

62.92 and 66.29 by different averages as reflected. In addition to the convergency, it must be observed that the absolute quantity reflected is less in every succeeding set, which I attribute to cleaning, (perhaps too particularly,) the speculum, though only with soft leather every time before commencing, and I have before observed the same, that specula soon lose the highest reflective power which they had when just polished. This shows the necessity of having them recently and highly polished for this purpose; it appears also, and I have no doubt further experiments will confirm it, that the quantity at smaller incidences is more affected than at the greater ones, which I should have expected, for the scratches and other faults are always less visible when the speculum is viewed obliquely.

The conclusions we are led to by the above experiments, are so contrary to the general opinion of philosophers, and to that of so many great authorities, that to be more certain of its being a general law in metals, I wished to compare also some other highly reflecting one, and accordingly, on the 1st and 2d February, reground and polished a small oval speculum of cast steel, and of the same dimensions as the former ones. On the 8th, I tried it in the photometer and obtained the following.

Angle of incidence.	Average of trials.	Light direct.		Light reflected.
10°	5 each,	$38\frac{11}{16}+2 = 40\frac{11}{16}$	equalled	$24\frac{31}{80}+7\frac{4}{15} = 31\frac{157}{240}$ in.
30°	3	$37\frac{5}{12}+1\frac{19}{20} = 39\frac{11}{30}$		$26\frac{23}{48}+3\frac{15}{22} = 30\frac{85}{528}$
50°	5	$38\frac{1}{16}+3\frac{1}{12} = 41\frac{7}{48}$		$26\frac{47}{80}+3\frac{11}{12} = 30\frac{121}{240}$

and

10° incidence $(40\frac{11}{16})^2 : (31\frac{157}{240})^2 :: 100 : 60.52$ rays reflected.

30° $(39\frac{11}{30})^2 : (30\frac{85}{528})^2 :: 100 : 58.69$

50° $(41\ \frac{7}{8})^2 : (30\frac{121}{240})^2 :: 100 : 54.96$

Those as well as another set taken on the 9th are more irregular than the former ones, with the mixed metal speculum, so also are the following taken on the 15th February.

Angle of incidence.	Average of trials.	Light direct.	Light reflected.
10°	2 each,	$37\frac{1}{32}+2=39\frac{1}{32}$	equalled $22\frac{1}{4}+7\frac{4}{15}=29\frac{31}{60}$ in.
20°	3	$37\frac{43}{48}+1\frac{1}{5}=39\frac{23}{240}$	$25\frac{11}{48}+3\frac{14}{15}=29\frac{13}{80}$
30°	3	$38\frac{1}{16}+1\frac{19}{20}=40\frac{1}{80}$	$26\frac{1}{8}+3\frac{15}{22}=29\frac{71}{88}$
40° not taken			
50°	3	$37\frac{13}{16}+3\frac{1}{12}=40\frac{43}{48}$	$25\frac{15}{16}+3\frac{11}{12}=29\frac{41}{48}$
60°	3	$38\frac{3}{16}+3\frac{7}{10}=41\frac{71}{80}$	$26\frac{11}{16}+4\frac{17}{60}=30\frac{233}{240}$

and

10° incidence $(39\frac{1}{32})^2 : (29\frac{31}{60})^2 :: 100 : 57.18$ rays reflected.
20° $(39\frac{23}{240})^2 : (29\frac{13}{80})^2 :: 100 : 55.64$
30° $(40\frac{1}{80})^2 : (29\frac{71}{88})^2 :: 100 : 55.49$
40°
50° $(40\frac{43}{48})^2 : (29\frac{41}{48})^2 :: 100 : 53.29$
60° $(41\frac{71}{80})^2 : (30\frac{233}{240})^2 :: 100 : 54.66$

The irregularity observable in the above is no doubt owing to the badness of the measurements, and they show no reason for supposing that steel follows a different law to the mixed metal.

I have since thought that this badness arose from the fibres of the wicks being acted upon by the oil, and they had not been renewed. I was quite as careful in taking them as those of the mixed metal speculum. On the 22d February I commenced a fresh set, but found the lights so bad, and the speculum evidently so much deteriorated, that, after trying to get better measurements for near two hours, I gave them up. An average of 14 of the best measurements, viz. 7 direct and 7 reflected, gave only 55.68, as reflected out of every 100 at 10° incidence, showing that steel, as well as speculum metal, soon loses its highest power of reflection, though it possesses so much more hardness and tenacity. Between the 15th and 22d February, it had only been twice in the telescope, and cleaned each time. As I had only ground and polished it in the common way for flat surfaces, I was not certain that it might be truly plane, and thought necessary to prove it on some astronomical objects. Accordingly, on the 19th February, with it and a $5\frac{1}{2}$ inch speculum of my own workmanship, of about 50 inches focal length, and with a power of 100, I saw α Geminorum beautifully and distinctly defined; and with a power of 150 saw γ Leonis to be double at the first view, which I think will be allowed to be a sufficient test of its surface being nearly plane.

It has been generally and frequently asserted, that the quantity of light reflected by metals is directly as their density, but this view is clearly untenable; for, though the quantities reflected by steel and speculum metal are nearly in the ratio of their densities, yet, if we pursue this rule, and apply it to gold and platinum, whose specific gravities are about three times that of steel, we shall find that they ought to reflect much more light than they receive, which is absurd. But I find that the quantities of light which are absorbed or lost in the two metals which were the subjects of the foregoing experiments, when the rays are incident nearly perpendicularly, are almost exactly in the ratio of their specific heats, taken for equal bulks.

It is a highly interesting inquiry to learn whether this will hold as a law with the other metals. At present we know so little respecting the forces concerned in producing the phenomena of reflection, that every theory must be mere hypothesis which would show why highly polished metals should reflect most light when incident perpendicularly, and what should be the law when incident obliquely.

Description of the Photometer.

The photometer consists of an upright screen *a b*, Figs. 6 and 7, about forty inches in height, from about the middle height of which projects the lateral piece *b c*, Fig. 6, or *b c f*, Fig. 8; which is about four and a-half inches broad and fifty inches in length, and supported near the farther end by a leg as at *g*. In the middle breadth of this lateral piece is fixed an upright pasteboard, *b d e*, Fig. 6 and 7, of four inches in height, and in length the same as the lateral piece. The two lamps are on the lateral piece, one on each side of the pasteboard, which keeps separate the light from each lamp. There is an aperture cut in the screen, as at *b*, Fig. 7, four inches broad and two and a-half inches high, which is covered over with thin paper, and when the lamps are lighted, each half of this paper is illuminated by its own lamp, and the shadow of the thickness of the pasteboard appears like a dark line between them, as at *b*, Fig. 2. When the illumination is not nearly equal from each lamp, the difference is seen very distinctly and beautifully upon this thin paper when viewed from behind the screen.

To avoid the necessity of going frequently into the stronger light, which prevents the eye for some time after from judging of small differences, and to enable me to go through the experiments with more speed and certainty, I placed the lamps on slides made of thin slips of wood, with thicker pieces at their farther extremities. The ends of the slides are seen on each side of *b*, Fig. 8, so that while observing the illumination of the paper behind the screen, I could draw the lamps nearer to, or push them farther from it; and having the right hand slide marked with inches and one-fourth inches to the commencement of the thicker piece of wood at its farther extremity, on which the lamp was placed, and to which the part carrying the reflector was attached, I could read off this distance also without leaving my seat behind the screen, by using a dark lantern.

Fig. 9 is a larger plan of the piece of wood at the extremity of the right hand slide, showing how the different angles of incidence are obtained by placing the lamps at pins fixed in the wood for that purpose, which give the required angle at once when the flame is seen in the centre of the speculum through a small hole bored in the screen for that purpose, just above the centre of that half of the thin paper, the speculum being fixed to an arm, which turns round the pivot *f* as a centre, being at the angle 45°, as used in the telescope for convenience. For a shade to intercept alternately the direct and reflected light, I have a square piece of wood which turns on a pivot at *b*, and into it are fixed two upright pieces *a b*, *b c*, at right angles to each other, and to the square piece. When *a b* is in the direction of the screen, the reflected light is intercepted; and when *b c* is in that direction, the direct light is intercepted; and I can turn it in either direction by pulling one or other of the strings which are fixed to its opposite corners, and brought to the screen so as to be nearly tight when the lamp is at the utmost distance required, see *h*, Fig. 8.

The upright pasteboard must be well blacked on the left hand side, and the right hand side covered with black velvet, to prevent all interference of foreign light, and so also must the shade which intercepts the light, and all the parts about the right hand lamp must be well blacked, as well as one-third of the length of the slide; and if any other precautions are necessary, they will soon suggest themselves to any one who is desirous to have correct results.

I have found the variation of the light of the lamps much diminished by using four small wicks, as shown in the figure, with a larger hole in their centre to admit air to the inside of the flame. Perhaps if more and smaller wicks still were used it would be better. The wicks should not be used if they have been more than a day or two in the oil. I believe now that the irregularity of the measurements taken with the steel mirror, is entirely owing to the wicks having remained so long as to be acted upon by the oil.

The flame must not be larger in proportion to the speculum, than so that the latter may reflect the light from every part to the screen when they are most distant from each other.

I found some difficulty at first in judging of the equality of the illuminations when I used a speculum of the common metal, from its giving the light a yellow tinge: but by tingeing the light of the other lamp to an intermediate shade, by a small piece of pasteboard stuck behind it, and painted of the requisite colour, the difficulty is done away with.

The best sort of paper I have found to receive the light upon in the screen is the thin unsized paper which is used to take copies of letters under the press, and it must be stretched tight over the aperture by moistening before pasting it to.

It will be seen that the divisions commencing only at the thicker piece of wood, the distance of the lamp in the direct measurements, and the sum of the distances of the lamp to the mirror, and the mirror to the commencement of the divisions, must be added afterwards in the reflected ones.

On the law by which the reflective power varies at different incidences in polished metallic surfaces.

The quantities of light reflected by metals, as we see by the experiments, have a particular relation to each other, the differences evidently converging for equal differences in the angles of incidence as we increase those angles.

If we form a geometrical construction, like the accompanying one, Fig. 10, taking the point O in the line P Q for a centre, and describe the circular arc R Q a quadrant, with radius O R or O Q = 100, and if we divide this quadrant into arcs of 10° each, and draw radii O 10°, O 20°, O 30°, &c. to these points in the circumference, we may consider the light to be incident at O at these angles; perpendiculars being let fall upon the radius O Q, the distances O *s* 10°, O *s* 20°, O *s* 30° &c. will be the sines of incidence to radius 100.

If now on the perpendiculars, produced if necessary, we set off the quantities of light reflected at the various incidences in the measurements for speculum metal taken 25th January, we find these terminate at *a b c*, and these points are almost exactly in a right line.

To subject this to calculation, and prove how near it agrees in numbers with those of the experiments, we must take the analytical expression for a right line, $y = ax + b$, then the lines O R, O Q will be the axes of the co-ordinates; the values of y will be the proportion of light reflected, radius being taken as the quantity supposed to be incident, and x will be the sine of incidence to that radius; b will equal the quantity reflected when incident perpendicularly, a being the trigonometrical tangent which a parallel to this line drawn through the point O makes with the axis of x.

In the experiments of the 25th January, b is about 72.3, and a is trig. tang. of about 355° 12′. From these data we find:—

y at 0° incidence	= 72.3	y at 50° incidence	= 65.87
10°	= 70.85	60	= 65.03
20°	= 69.43	70	= 64.41
30°	= 68.11	80	= 64.04
40°	= 66.91	90	= 63.91

which we see is as near to the quantity determined by experiments as can be expected from the nature of them.

It is also remarkable, that, in the experiments of the 26th and 27th, the quantities still form right lines, though the reflective power was diminished; the last measurements in those of the 26th and the first in those of the 27th, being the only ones showing anything of irregularity, and this not more than must be allowed for error in photometrical investigations.

Hence I deduce this law, that the reflective power in metals of smooth surface is a function of the sine of incidence containing two constant quantities, which have different values for different metals, depending on the peculiar nature of each.

We are at present ignorant of the properties which produce the forces, whose effects are represented by *a* and *b* in the above formula, and even whether they have any dependence on each other; but we may conclude, from the simplicity of the law, that the case of reflection by metals is of the simplest order.

ART. X.—*On the Double Chlorides of Gold and Potassium, with a Note on the Tartaric Acid of the Vosges.* By Professor BERZELIUS, in a Letter to the EDITOR.

SIR,

IN the 5th Number of the *Edinburgh Journal of Science*, which I have just received, I find in page 138 the result of an analysis of the double chloride of gold and potassium, which I had the honour of making conjointly with your countryman Mr Johnston, which differs essentially from the result of an analysis made by Mr Johnston after his return to Edinburgh, and from which he concludes that there may exist two different double chlorides of gold and potassium. I consider it my duty to explain this point. Mr Johnston having informed me some time ago of his suspicions as to this matter, I have examined the data obtained by the analysis he describes, and I have found, that, by an error of subtraction, the weight of the chlorine was diminished by a quantity, which, by the same error, was added to the chloride of potassium. The weight of the gold is what it ought to be in the anhydrous salt. I have since made a new analysis of the same double salt, of which I take the liberty of communicating to you the result.

Chloride of potassium,	17.525 = 1 atom.
Metallic gold, -	46.800 = 1 ——
Chlorine, - -	25.050 = 3 ——
Water, - -	10.625 = 5 ——

This analysis differs a little from that of Mr Johnston, in assigning to this salt an atom more of water, Mr Johnston having found only four. I would not dare to insist on the preference of my result, for this salt is very efflorescent, so that in endeavouring to dry it perfectly, that it may contain no uncombined moisture, we risk a partial efflorescence; while on the other hand, in avoiding efflorescence, we are never sure of having it perfectly dry.

To ascertain if there be another double chloride of gold and potassium, I have made different experiments, which seem to prove, that the chloride, proportional to the *per*-oxide, combines in only one proportion with the chloride of potassium, but that there is also a double chloride in which the chloride of potassium is combined with the chloride of gold, proportional to the protoxide of gold, (the protochloride). It is formed by melting the preceding. The melted mass is brown and pellucid at the edges, resists for a long time a bright red heat in close vessels, and leaves much metallic gold undissolved when treated with water or muriatic acid.

If you think this explanation of the results communicated in your *Journal* by Mr Johnston will interest your readers, I beg you to do me the honour of inserting them.—Accept, Sir, the expression of my very high regard,

JAC. BERZELIUS.

Postscript.—I have newly finished a research which has given me some very curious results. I wished to examine the difference between the analytical results obtained from tartaric acid by Dr Prout and myself,* and at the same time I have analyzed an acid found in some kinds of tartar, and examined by John, Gay Lussac, and others, who have all determined positively that it differs from the tartaric acid. In the analysis I have made of it, I found that it has the same composition and the same atomic weight as the tartaric acid, from which it differs precisely as the pyro-phosphoric does from the phosphoric, or the cyanous from the fulminic acid. The examination which I have made of these bodies having an identical composition, but possessing different properties, leads to a research entirely new to that of *heteromorphous* bodies composed of the same number of atoms of the *same elements*, but combined among themselves in a *different manner*, just as the experiments of Mitscherlich have discovered *isomorphous* bodies composed of the same number of atoms of *different elements*,—but combined in an *analogous manner*.

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* This difference amounted to one volume, or half an atom of hydrogen; and Berzelius's new result, as expressed in a letter to Mr Johnston, agrees entirely with the accurate result of Dr Prout.

Stockholm, 12*th July* 1830.

Art. XI.—*On the double Chlorides of Gold with Potassium and Lithium.* By James F. W. Johnston, A. M. Communicated by the Author.

I. *Potassium Salt.*

In a paper on the double Chlorides of Gold, published in the preceding number of this *Journal*, I inserted an analysis of the potassium salt I made in Berzelius' Laboratory with great care, but differing very materially in its results from a succeeding analysis performed by myself. Before publishing either analysis, I communicated my results to Berzelius, and should have waited his reply, had he not informed me previously that he had already made known his results to the Swedish Academy. Soon after my paper was published, however, I received a letter from Berzelius, informing me that my analysis was substantially correct,—that he had verified it by a *new* analysis of his own, and had found, on looking back to his note-book, an error of subtraction, by which the calculated result of his former analysis had been vitiated. This new analysis and correction I had intended, in justice to Berzelius, to make known in this *Journal*, but the reader will find the whole matter explained in the letter forming the preceding article, which Dr Brewster was kind enough to transmit to me.

The new result of Berzelius differs from mine only in assigning about 1½ per cent. more water to the constitution of the salt. My result gives 9.13, and that of Berzelius 10.62 for the per centage of water; he therefore deduces five atoms for that contained in each atom of the salt. On learning his result, I felt that I could not insist upon this point, and for the reason he has stated, though, except when in very thin scales, I have not found the crystals effloresce so very speedily as they seem to have done in his probably drier laboratory at Stockholm. I have, however, made two other experiments, with the view of determining the point.

24.76 grs. of the crystals, partly prisms, and partly large plates, after drying on bibulous paper, and before the slightest trace of efflorescence had shown itself, were heated on a water bath for an hour, when the loss amounted to little more than eight per cent. Heated more strongly to incipient fusion, the loss amounted to 9.26 per cent. By a third heating the ultimate loss was 2.3 grs. = 9.4 per cent. One portion of the salt in this state being fused in a tube over a spirit lamp gave still a trace of moisture, while another portion treated with water, left a small insoluble residue of proto-chloride of gold. From this experiment we should infer that 9.4 was very near the true per centage of water in the salt.

Again, 20.21 grs. in fine prismatic needles were dried by pressure between folds of paper, and when they had the appearance of being dry, were taken from the still moist paper and weighed. By drying at 212°, they lost only 8.4 per cent, but by heating more strongly till an odour of chlorine became perceptible, the loss reached 1.91 grs. = 9.45 per cent. In this state they still dissolved in water without the slightest residue, and gave a trace of moisture when fused in a tube. This experiment would lead us to suppose that the true amount of water exceeded 9.45 per cent. In the state of prismatic needles, however, the salt is not so well adapted for experiment as in that of large crystals, and I have reason to think, that the crystals employed in both these experiments still retained a portion of moisture mechanically attached. Assuming, therefore, 9.45 per cent. for the quantity of water contained by the crystals, a quantity exceeding my former determination by .32 grs. the several analysis of this salt hitherto published will stand as follows:

		Javal.	Berzelius.	Johnston.
Chloride of potassium,	1 atom,	24.26	17.525	18.38
Chlorine,	3	68.64	25.050	25.440
Gold,	1		46.800	46.73
Water,	4 or 5 atoms,	7.10	10.625	9.45
		100	100	100

There is a curious property of many salts which I have seldom seen adverted to, but which is nevertheless extremely deserving of attention. We commonly describe salts in their crystalline state only, and attend little to their habits in the amorphous state. And yet, in this state, they occasionally exhibit very interesting phenomena. Some salts, when deprived of their water by fusion, deliquesce on cooling, and run into a liquid in which crystals are afterwards gradually deposited till the whole has assumed the form of a crystalline salt, containing water, and permanent in the air. Others attract only so much moisture as to admit of an internal motion of the atoms, and assume a crystalline arrangement without becoming liquid, of which kind is common barley sugar, as has been shown by Mr Graham. The chloride of gold and potassium is an efflorescent salt, and when left to itself gradually falls to powder. Yet the large crystals employed in the former of the two experiments above detailed, after heating to the fusing point, and losing by that means upwards of nine per cent., being set aside for a couple of days, had reached at the end of that time within half a per cent. of their original weight. Such an attraction for moisture we are not prepared to expect in an efflorescent salt, but all these phenomena are probably due to a tendency, with which I consider all matter to be endowed, to assume regularly crystallized forms, and to attract to themselves such neighbouring substances as may aid that tendency. This proneness to crystalline arrangement, however, is not to be recognized as any new principle, but simply as an uniform result of universal mechanical laws.

II. Lithium Chloride of Gold.

When I drew up my former paper I had not formed this salt. I have since prepared a small quantity of it, and am enabled to describe its external characters. I had in my possession a quantity of sulphate of lithia, prepared by Arfvedson, from which I formed a quantity of chloride, by means of barytes. This was mixed with chloride of gold, and the liquor carefully evaporated. Crystals of the potassium salt first separated, easily recognized by their appearance, and by their efflorescing property, showing that the sulphate of lithia had not been purified from potash. After two or three careful evaporations these ceased to be separated, and a crop of minute prismatic golden-yellow needles was obtained, capable of removal, but not of drying between folds of paper, owing to their rapid deliquescence. In the air they speedily dissolve into a yellow liquid. At 212 it becomes opaque and parts with its water, and by the flame of a spirit lamp is wholly decomposed, giving off chlorine, and leaving metallic gold with the deliquescent chloride of lithium. Were the preparation of this salt not too expensive a process, it might be recommended as a very effectual one for separating the two alkalies, potash and lithia; experiment, however, may probably indicate other triple salts by the different solubilities of which the same object may be satisfactorily accomplished.

I have not made any analysis of the lithium salt, as I consider the analyses already given of the other salts sufficient to establish the theoretical composition of all these combinations. The water of crystallization alone is wanting, and that the avidity of this compound for atmospheric moisture prevented me from attempting to ascertain.

I shall subjoin here a view of the *distinctive* characters of the four salts of gold with the alkalies.

1. The *sodium* salt is permanent in the air. The *potassium* and *ammonium* salts efflorescent, and the *lithium* salt rapidly deliquescent.

2. The *sodium* salt requires a strong heat and length of time to dissipate its water of crystallization. The *potassium* salt parts with all its water at a heat a little above,—the *ammonium* salt something below, and the *lithium* salt about that of boiling water.

3. The *poassium* and *ammonium* salts are easily distinguished by the readier efflorescence of the latter, and by its

giving off at a gentle heat not only chlorine, but white fumes of sal-ammoniac.

4. All the salts crystallize in four-sided prismatic needles, but in this state the *sodium* salt is readily recognized by its peculiar aspect, giving an impression as if the crystals were all longitudinally striated. In larger crystals it is denser than the other compounds, and darker coloured; emitting a distinct metallic sound when dropped upon glass.

5. Left to spontaneous evaporation the predominating form of the *sodium* salt seems to be flat rhombic prisms, or large regular rhombic tables forming flatly on the bottom of the vessel. Of the *potassium* salt, rhombic prisms, and from a solution containing excess of potash, large beautiful pale yellow prisms supported edge-ways, and increasing upwards into the fluid. The *ammonium* salt undergoes more modifications. It forms large rhombic tables or thin plates, increasing upwards after the manner of the potassium salt, or four-sided, modified at the edges into six-sided rectangular prisms. The *lithium* salt in prismatic needles has something of the aspect of the sodium salt in the same form; but it soon loses this aspect by its rapid deliquescence.

Dr Brewster informs me that all these salts possess the remarkable property of dichroism; and gives me reason to hope that he will examine their crystallographic and optical characters more nearly as soon as leisure permits.

PORTOBELLO, *23d August* 1830.

ART. XII.—*The most ancient form in which Gold was made use of in Scotland as the current money of the times.* Communicated by S. HIBBERT, M. D. F. R. S. E. &c.

ABOUT four or five years ago the following notice appeared in the Inverness Courier:—

"In ploughing up a field at Leys, near the town, the ploughman found a rod of pure gold, about fifteen inches long, with three sides, each about half an inch in depth. In the middle it is twisted, and terminated by a bend similar to a shepherd's crook, in very rude workmanship. It was purchased at L. 4, 10s. by Mr Naughton, jeweller here, and is now in his possession. No one who has seen it can imagine the use to which it may be applied; but we hope that the owner of it will not send it to the crucible till it is examined by some of our antiquarians."

After this notice had appeared, Sir Henry Jardine kindly submitted the rod to the inspection of the Society of Scottish Antiquaries at one of their meetings, in which I was present as Secretary; and on the same occasion was read a short notice of it by Mr George Anderson, F. R. S. E., the active and able Secretary of the Northern Institution of Inverness.

"The only additional information I have been able to procure," says Mr Anderson, "respecting this discovery is, that the plough seems to have gone several times over the spot where the treasure lay before it was noticed, and that the bend in the largest portion of the rod is thought by some who have seen it to have been occasioned either by the horses' feet or by the ploughshare." It was also added, that "two or three small pieces subsequently found, were also brought to the jeweller, forming, when joined together, a gold rod of about 18 inches in length, nearly half an inch in thickness, three-sided or grooved, and spirally twisted."

The information thus conveyed, in connection with the actual relic which was exhibited, showed that it was nothing but a plain spiral gold rod, perhaps originally unbent, devoid of all ornament whatever.

Regarding its probable use, much was said on this occasion. Mr Anderson communicated the opinion of a learned gentleman, the author of the Culloden papers, who imagined that all the pieces found must have formed portions of a bent wire or rod for suspending the sacrificial vessels of the Druids.

Another opinion hazarded by Mr Anderson, though with proper diffidence, and in which I myself was disposed to concur for want of a better conjecture, was, that the gold rod might have been an ensign of office, or some mark of distinction worn by a chief for superior wisdom or valour, analogous to the golden torquis or collar worn by chiefs of many ancient nations, or to the wreath of gold (torch aur) said to have been particularly in use among Welsh leaders of distinction.

The discouragement, however, to this last supposition, was, that there was a great presumption that the gold relic never existed in the form of a collar, and that, being devoid of all ornament, and in the simple form of a spiral rod, it could not possibly have been devoted to this purpose. Nor was the probability more in favour of the first mentioned opinion which assigned its use to the Druids; proofs being equally wanting that the Druids ever made a sacrificial use of rods of this kind, or that this order of Celtic priesthood had ever found a way to the distant lands of Inverness. In short, the antiquities of this vicinity, too confidently imagined to be Druidic, are rather assignable to the Scandinavians, who formed a settlement on this coast.

But it is now time to advance my own theory, which is simply this: That the said rod of gold, found near Inverness, indicated nothing more than the form of the current money of many northern countries, when paid in gold. Thus we find that in the Welsh laws of Howel Dha, it was enacted, that for invading the prince's bed the offender was "*to pay a rod of pure gold of the thickness of the finger of a ploughman, and in length from the ground to the prince's mouth when sitting.*" And in another part of the said code, it is enjoined, that the fine for insulting the king of Aberfraw shall be paid as follows: a hundred cows from every hundred in his lordship; *a rod of gold as long as himself and as thick as his little finger*: and a dish of gold as broad as his face, and as thick as a husbandman's nail, who has been a husbandman for seven years."

From these quotations the use and nature of the unornamented spiral rod of gold, found at Inverness, is so self-evident, as scarcely to need a single comment. But if any suspicion should exist that it was still more intended for ornament than as a species of currency, I am fortunately enabled to quote an extract from the Rev. James Johnstone's translation of the Norwegian account of Haco's expedition against Scotland, A. D. 1263, to show, that while a valuable rod of this kind was converted by the same Northmen, who peopled many parts of North Britain, to the purpose of money, it was acknowledged to convey a twofold signification, being, from its flexibility, and superior worth as a metal, rendered a badge of power. A rod of gold which could be thus twisted was named a ring, and Johnstone states in a note, that *Ringa* (the word quoted in an ancient Saga) " not only signified rings or bracelets, but also money; for before the introduction of coinage into the north, very thick spiral gold wires were worn round the wrists of great men, who distributed *bits* to those who performed any signal service; and such a wire is still to be seen in the Royal Museum of Copenhagen. It is not always easy to discern when by *ringa* is understood ornaments for the fingers, bracelets, rings of investiture, or the current money of the times."

The use of the spiral rod of gold found near Inverness, the ancient resort of the Vikingr, is at length rendered manifest. It was the current money of Scotland, which the chief was wont to distribute in *bits* to his followers, and when twisted manifold around his wrist, served as a badge of opulence and distinction. This is perhaps one of the most curious antiquarian relics which has been found in Scotland for many years. I am ignorant of its fate, whether it has met with preservation in the Museum of the Northern Institution of Inverness, or has been committed to the crucible. I regret at the time I saw it my ignorance of its use, and of the interest which it was thereby calculated to excite, as showing the most ancient form in which gold was made use of in Scotland, as the most valuable of currencies.

Art. XIII.—*Remarks on the alleged Polarization of Heat.* By the Rev. Baden Powell, M. A. F. R. S., of Oriel College, Savilian Professor of Geometry in the University of Oxford, and Honorary Member of the Society of Arts for Scotland. Communicated by the Author.

The analogies between light and radiant heat have formed a favourite topic of inquiry and discussion to natural philosophers: and have certainly in several instances afforded the means of guiding them to the discovery of important properties of heat. At the same time it must be admitted, that the points of analogy have in many cases been mistaken, and hence much vague and hypothetical speculation has arisen. Not a little perhaps of such speculation has been occasioned by the confused ideas long adopted as to the nature of the heating agents described under the general name of "radiant heat," but which I conceive later experiments have tended to dispel, by analyzing the effect so designated into its component elements, and thus furnishing us with more precise notions, by the adoption of which we may view the various results already obtained in a new and clearer light, perceive their relations and analogies presented in a more just point of view, and thus possess a more sure foundation for further researches.

The discovery of the polarization of light by reflexion, and the law of its subsequent non-reflexion at the proper incidence when the planes of reflexion are at right angles, exhibited by the experiments of Malus, afforded a new field of inquiry to those who were engaged in pursuing the analogies of light and heat. Accordingly, the subject was taken up by M. Berard, with the view of trying whether a similar property was possessed by "radiant heat," using the term in the somewhat vague sense in which it was then employed.

It is to be regretted that of his experiments we possess no further accounts than very general abstracts. Berard's memoir was read before the French Institute, and in the *Annales de Chimie* for March 1813, is given the report of the Commissioners of the Institute upon it, of which a translation appears in the *Annals of Philosophy*, O. S. vol. ii. p. 164. The substance of the same statement is also given by Biot in his *Traité de Physique*, vol. iv. p. 602. In this report the Commissioners recommend the memoir to be printed in the "*Recueil des Savans Etrangers*," but, as far as I have been able to learn, it has never yet been printed. The title of the memoir is, "*On the Physical and Chemical Properties of Solar Light.*" In the sequel, the author is led to examine the polarization (as he considers it) of the solar heat by reflexion, as in Malus's apparatus, collecting the rays reflected from the second glass by a concave mirror having a blackened thermometer in the focus, adjusted so as to receive the rays at the proper angle, and fixed so as to move round in azimuth with the second glass, and to continue in an invariable relative position to it.

With the same apparatus he is stated to have tried the effect, when, instead of the solar rays, was substituted the radiant heat from a mass of hot metal, "à peine rouge, ou même tout à fait obscur," (Biot, iv. 611.) In both cases the result was, that in the same positions in which light was reflected or not from the second glass, the heat was likewise reflected or not.

Upon finding the distinction between the two species of heating agents, which I conceive is established in my paper, (*Phil. Trans.* 1825, Part i.) * I was naturally led to examine the above statements respecting polarization in connection with that distinction.

So far as respects *the solar rays*, it having been, as I conceive, clearly established that the solar heat is of one simple kind, viz. of the species distinguished by passing through transparent media without heating them, inseparable from the rays of light, and only developed upon the absorption of those rays by black surfaces, Berard's results appeared to me to amount to this, viz. that the solar *light* when polarized still retains its *heating power* unaltered; and, accordingly, whether the light is reflected or transmitted by the second glass, according to its azimuth, the heating power is of course conveyed with it as in all other cases. A contrary result would have been an extraordinary exception to the general law.

In regard to the effects derived from hot metal, I could wish the results had been stated in a more specific and detailed form. If they be understood as applying to the case of hot metal possessing any degree of luminosity, it is certainly conceivable that the portion of the effect due to the light may have been sufficient to exhibit a minute effect precisely analogous to the case of the solar light: with regard to *metal absolutely non-luminous*, and to the other portion of the effect, viz. *the simple heat* in the case of *luminous hot metal*, the main question would arise. To show that *simple heat*, distinct from light, exhibits this modification would be a point of the highest interest: and this Berard is stated to have done. Supposing the fact to be as recorded, we must carefully observe *how far the ana-*

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* See Note at the end.

logy extends, and this is merely to the circumstance of *non-reflexion* in the proper azimuth. There is nothing to show whether the heat which was not reflected was *transmitted* like light, (which would be a remarkable exception to the general law,) or whether it was absorbed by the glass, which would render the phenomenon essentially different from the polarization of light.

The great importance of Berard's result, as referring to *simple heat*, makes it a matter of extreme regret that we should have no further account of it than so very general a statement as that just cited. It since became a point of the greatest interest to me to endeavour to verify such results—to examine closely into all possible sources of deception,—and to attempt to remove any uncertainty or ambiguity which might be supposed to affect the question from want of due distinction between the different species of heating agents. Immediately on the termination of my former inquiries, therefore, I commenced a set of experiments on these points; but the great difficulties I found in carrying them on, or, to speak more precisely, in obtaining any results at all, together with other causes, have delayed and protracted the prosecution of the inquiry till a very recent period, and even now the results are far from being as satisfactory as I could wish. I will proceed, however, to a brief account of my attempts, from which a tolerable judgment may be formed how far I may be considered to have substantiated my conclusion, which goes to contradict that of Berard so far as simple heat is concerned. I am fully aware of the difficulty of proving a negative, and especially when in opposition to such high authority; but in the absence of any detailed account of his experiments, my attempts may not perhaps appear wholly devoid of interest.

The apparatus employed for these experiments, was precisely similar in principle to that used by Berard. It consisted of a tube having at each end a plane reflector of plate glass inclined to the axis at about an angle of $35\frac{1}{2}°$, and capable of adjustment by a screw; the tube consisted of two parts sliding one in the other, so as to give to the second glass a motion in azimuth. To the frame bearing this second glass was fixed an arm carrying a small concave metallic reflector, and a side branch holding a small mercurial thermometer, which could be adjusted so that its bulb should be exactly in the focus of the reflector; and this reflector fixed in the requisite position to receive the rays from the glass reflector at the polarizing angle.

When the apparatus was in use, therefore, a source of radiant heat being placed in a proper position with respect to the first glass, the rays of heat would be reflected along the axis of the tube, and impinging on the second glass would be again reflected from it; and being intercepted by the concave reflector, would be made to converge at its focus, and thus concentrated, would fall on the bulb of the thermometer. The apparatus having been adjusted for light, the observations were to be made for heat in the first instance, with the two glasses in the same azimuth, that is, with the planes of both the first and second reflection coincident. The second glass was then to be turned round in azimuth, through a quadrant; so that the two planes of reflexion should now be at right angles. If then the heat had acquired by its first reflection a property of polarization analogous to that which light requires under the same circumstances, we ought in the first position of the second glass, that is, in azimuth 0°, to perceive an effect on the thermometer, and at azimuth 90°, to observe none, or a gently diminished effect.

In applying these considerations to practice, however, there are a multitude of circumstances to be attended to, which materially interfere with the results. Into the detail of these circumstances I shall not attempt here to enter. It will in general be sufficiently evident that among the first objects of attention must be the exclusion of irregular radiations, currents of heated air, &c. This can never perhaps be perfectly effected. The reflectors become heated; the different positions of the second glass in azimuth may place it and the thermometer in a different situation with respect to some or other of the heating causes. I have in many preliminary trials been greatly misled by effects due to circumstances of this kind. But it is impossible to detail them in a paper like the present; and the general statement must suffice to assure the reader, that no caution was spared in guarding against these fallacies, and to warn those who may repeat the experiments against their influence. The main difficulty with which I had to contend in these experiments is *the very small total amount of the heat which in any case arrives at the focal thermometer.* This small effect, even in the most favourable instances, was unavoidably more or less disguised by interfering causes; and the difficulty of appreciating the difference, if any, due to a change of azimuth, was such as to render the most numerous repetitions necessary before any degree of confidence could be placed in the results indicated. The thermometer was a very delicate one constructed on purpose: its scale was such as to admit of a centigrade degree being divided into 5ths, and the 10ths are readily estimated. The bulb is about one-fourth inch diameter: detached about one inch from the mounting, and coated with Indian ink.

My object was to try experiments on the principle thus described in the case of simple radiant heat from non-luminous sources, as well as in that of the compound radiation from luminous hot bodies. In this latter case the one species of heat could be separated from the other by the interposition of a glass screen, (which was usually placed about half way in the tube,) and the modifications which each portion might undergo could be separately discovered.

The direct experiment was to be made (as I have already described) by means of the heat *reflected* from the second glass: but comparative experiments might also be made on the heat *transmitted* through the second glass, if any; and this, in the position of non-reflexion, ought, if the analogy with light hold good, to be displayed in a maximum effect on the thermometer in that azimuth, if the concave reflector with the thermometer in its focus were placed *behind* the second glass; and in several series of experiments this arrangement, which may be called *complementary* to the former, was adopted. And this point was further pursued by observing the *temperature* acquired by the *second glass*, by means of a small thermometer in contact with it, in order to find whether it *absorbed* without transmitting more heat in one azimuth than in the other.

The experiments were tried upon the radiations from the flame of an argand lamp, and from a ball of iron heated to the brightest incandescence which a common fire could produce, as well as at a point below visible redness. The total effects, as I have already observed, were in all cases but very minute. This was, indeed, to be expected; nor, upon a careful consideration of all the circumstances, did it appear practicable to obtain larger indications which would not be affected by proportionally increased causes of error. The simple radiant heat from the red hot ball after two reflexions becomes almost insensible, while the glass reflectors, especially the first, become heated, and the radiation from them disguises the result. The same may be said of the entire radiation from hot iron when non-luminous. The other part of the heat, viz. that belonging to the light, is excessively minute, but is less liable to be lost in reflexion. In the case of flame, this part of the effect would constitute nearly the whole after-reflexion, since the simple heat would hardly be sensible even after one reflexion.

Under these circumstances, after very numerous repetitions in each case, I was hardly surprised to find that, even in regard to the light, the most minute effects only could be recognized by the agreement of a long series of observed results. And these, as far as they went, were, as might be anticipated, entirely accordant with the idea that the *light*, being polarized, was, agreeably to the law of polarization, reflected or not at the second incidence, and its heating power of course accompanied it. Considering then *this portion of the results*, obtained by the interposition of a glass-screen, they would only show that in this case the term *polarization of heat is most improperly applied.* The *light is polarized*, and whether it be reflected or not at the second incidence, *it conveys the heating power with it*, which arises simply from its absorption by black surfaces.

With respect to *the other portion* of the effect, or *the simple radiant heat*, I can only say, that I have been unable, in all the lengthened series of results I have obtained, to perceive the smallest difference in the effects due to it, in the two rectangular azimuths of the second glass.

These effects, in the instance of non-luminous hot iron, were likely to be obtained in their simplest form; and it only remains to be considered how far the radiation from the glass reflector may have contributed to the observed effect. The increase of temperature of the second glass was observed and found very small; and if any non-reflexion of the regularly transmitted heat had occurred in the proper azimuth, it may, I conceive, be inferred, that it must have made some perceptible difference between the long series of results observed in two positions. *Not the smallest difference, however, was upon the whole perceptible.* Yet in the corresponding case of the *lamp*, such a difference did certainly appear.

With the luminous hot iron again, (the glass screen being interposed,) there was an appreciable difference between the two azimuths; but on the admission of the simple heat, by the removal of the screen, the whole intensity being of course increased, that small difference was in no degree increased with it.

How far results of this description can be considered as tending to decide the question under consideration, it is not for me to determine. But the distinction I have formerly pointed out between the *two species* of heating effect in luminous radiations, must be admitted to place the question in a different point of view from that in which it has hitherto been regarded. We have thus to regard Berard's conclusion, with respect to *simple heat, as the essential point; and this conclusion, (so far as my experiments are to be relied on,) I have uniformly failed in being able to verify.*

It only remains for me to express my earnest hope that some experimentalist, better qualified for the task, will be induced to take up the subject. And by the construction of better apparatus, and the invention and adoption of more satisfactory methods of experimenting, will succeed in bringing this interesting question to a completely decisive termination. It is, indeed, chiefly with the view of suggesting this, that I am induced to give publicity to these remarks.

NOTE.

Perhaps it may here, for the sake of some readers, not be irrelevant to state briefly the nature of my former experiments alluded to.

I conceive I have shown in those experiments that there are two distinct species of heating effect emanating together from luminous hot bodies. The one *transmissible through glass*, and affecting bodies according to their *darkness of colour*. The other *not transmissible through glass*, and affecting bodies according to the *texture of their surfaces.*

The solar heat appears to consist solely of the first kind: that of luminous hot bodies of both; and that of non-luminous hot bodies, solely of the second kind.

My paper, published in 1825, having been nearly a first attempt at an investigation of this kind, has many of the defects incidental to an early production. In particular, the experiments were complicated by several circumstances which further consideration shows to have been unnecessary.

The essential experiment is in fact one of extreme simplicity. It is only necessary to place a *smooth black*, and an *absorptive white*, thermometer near together at a short distance from a *luminous* source of heat; observe the degrees risen by one, while the other rises a given quantity as one degree. Repeat the observation with a glass screen interposed, (keeping them in the same position,) and the *inequality* of their risings will be found in all cases *increased.*

Thermometers of the most ordinary sensibility are quite sufficient; and the rays from a red hot poker, from the flame of a lamp, or even a candle, give very satisfactory results.

It will be found on consideration, that all the interfering causes are such as would tend to *diminish* the inequality when the screen is used, yet we find it invariably *increased.*

It may here be observed, that I have all along assumed as one of the characteristics of simple radiant heat the general fact of its incapacity to be transmitted in the way of direct radiation through solid media acting as screens. To this general fact, the experiments of Mr Ritchie make one exception, in the instance of transparent screens of great tenuity. Though some results which I fomerly obtained were at variance with his, yet, from what he has subsequently written, I am willing to admit the superior delicacy of his apparatus. And I may be allowed to take this opportunity (which various causes have before denied me) of acknowledging the candid tone of his remarks on my experiments. Relying on that candour, I may also be permitted to remark in general, that this exception appears to me so remarkable a one that I should be anxious to examine whether there may not yet be found some means of explaining it. The progress of research has tended to diminish the points of resemblance between light and *simple* heat. Even the fact of the radiation of heat itself seems very likely to possess little *real* analogy with the propagation of light. The mode in which heat is in general propagated through screens has been so satisfactorily traced, and shown to be so essentially distinct in its nature from direct radiation, as to make it a very extraordinary circumstance, that in this one instance the heat should possess a new mode of action: and more especially when this property is connected with the *transparency* of the screen; a quality to which heat is in no other case found to bear any reference. These considerations would show the propriety of a careful examination into every circumstance which is likely to afford any solution of the anomaly. Without pretending to enter at present into such an examination, I will content myself with barely suggesting one or two points which might possibly be further inquired into with advantage:—1*st*, The accuracy and fitness of air thermometers in researches of this nature. 2*d*, The laws by which the absorption and subsequent radiation of heat is regulated in bodies extremely small, and thence in screens of great tenuity, whether consisting of fixed substances, or perpetually renewed films of fluid. 3*d*, Whether the difference of the *conducting* powers of bodies continues to display itself when the substances are reduced to a state of great tenuity, as in the instance of a film of charcoal coating a glass screen. How far any of these causes may be found adequate to explain any part of the difficulty I do not in the least pretend to say. I leave the consideration of them to those who may be better able to follow up the inquiry.

ART. XIV.—*On Johannite, a New Mineral Species.* By W. HAIDINGER, Esq. F. R. S. E. &c. Communicated by the Author.

THE forms of Johannite belong to the hemiprismatic system.

I have observed only two varieties, which are represented in Fig. 12 and 13, Plate II.

Although the crystals are pretty regularly formed, and possess sharp edges, yet they are so very minute, and grouped together in botryoidal concretions, that it becomes very difficult to find out the true form, and still more so to measure the angles. The latter I succeeded only in measuring by approximation as follows: Inclination of a on a, adjacent = 111°, of a on b = 118°, of a on c' or a' on c = 87° 28′ of b on c = 128° 32′, of b on d = 134° 5′, of b on e (over c) = 101° 15′

I did not succeed without rather unlikely hypotheses, in ascertaining the dimensions of any pyramid, which might be considered as the fundamental form of the species. I have preferred, therefore, to put down the measures of the angles, as I obtained them by the application of the reflective goniometer; while larger and more complicated forms of crystals may be discovered hereafter, which may allow of a more easy and exact determination of all the geometrical relations of the series of crystallization.

On account of the smallness of the crystals, cleavage is observed with great difficulty; yet I perceived traces parallel to the faces marked a, also parallel to another face, which replaces the sharp edges between b and c; in other directions, there is imperfect conchoidal fracture.

The surface of the crystals is smooth, the faces b, d, c, e, are slightly streaked, parallel to their edges of combination.

The Johannite possesses vitreous lustre; its colour is a fine bright grass-green, which becomes pale siskin-green in the streak. The crystals are semitransparent.

It is sectile, the hardness = 2.0...2.5, rather more considerable than that of hexahedral rock-salt. The specific gravity I found = 3.191, at 59° F.

It is slightly soluble in water, and occasions a faint taste, more bitter than astringent.

Johannite belongs to the order *Salt*, in the first class of the system of Mohs. As it will become necessary in future to dispose, into genera and species, the whole contents of this order, and to apply consequently systematic denominations to them all, I shall not now by a hasty determination unnecessarily increase the number of such names. At all events, it does not belong to the genus vitriol salt. The denomination of *Vitriol of Uranium*, proposed by John,[*] recalls to our memory alchemistical ideas, which are long and deservedly forgotten.

It is with the highest gratification that I propose the name of *Johannite* for the present species; for no mineralogist ever had an opportunity, in paying a compliment to a distinguished patron of his science, to apply to a new species the name of the brother of his prince. I am indebted for this peculiar favour to his Imperial Highness the Archduke John of Austria. I have endeavoured to remind the latest of future admirers of one of his favourite sciences of a name, upon which we dwell with pleasure in the history of the present age, and thus to preserve, as long as the progress of science shall be attributed to the labours of our own contemporaries, the recollection of my regard to him.

The specimens which I examined I first saw at Joachimsthal, in Bohemia, when I visited that celebrated mining town in spring 1826 with Mr Robert Allan, in the collection of a mining officer, Mr Peschka. This collection having been purchased by Count Caspar Sternberg, and presented to the National Museum at Prague, I was fortunate enough to obtain the specimens for examination in spring 1829. I had long ago wished to give the name of Johannite to a species found in the Austrian dominions, and had likewise requested his Imperial Highness's permission to do so; and I found this species the more agreeable to my purpose, as its green colour contains an allusion to the Alps, the favourite abode of its imperial namesake.

I have been frequently indebted to Professor Zippe for various interesting minerals for examination. I am under particular obligations to him in the present case, he himself having already published several valuable papers, and the determination of a new species being particularly interesting.

The species itself deserves to be considered as new in mineralogy, although John has already published an analysis of it; yet both physical and chemical properties were so imperfectly described, that it is impossible to infer from them alone the identity of Johannite with his vitriol of uranium. I owe my full conviction of it, only to the verbal communication of Mr Peschka, whom I called for expressly for that purpose.

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[*] *Chemische Schriften*, Bd. vi. p. 254.

Johannite exposed in a glass tube to the flame of the spirit lamp, gives off a considerable portion of water, whereby a dark-brown residue is left, which is friable, and still shows traces of the original crystallization of the mineral.

When melted on charcoal with carbonate of soda, and placed on a bright surface of silver, and afterwards wetted, a black spot of sulphuret of silver is formed on that surface. Also a smell of sulphuretted hydrogen is disengaged. If kept somewhat longer in the reducing flame of the blowpipe, and then again melted with carbonate of soda in the reducing flame, globules of copper are obtained.

Johannite forms with borax a fine green glass, both in the oxidating and in the reducing flame. In the latter, the globule sometimes also appears red and opaque on cooling, from the protoxide of copper.

When treated with salt of phosphorus, only the green tints appear, owing chiefly to copper in the oxidating flame, and to uranium in the reducing flame. By a long continued blast of the reducing flame, the globule becomes covered with a black metallic surface, if much of the Johannite has been employed. By an addition of tin, the red colour of the protoxide of copper is obtained.

In a solution of Johannite in nitric acid, caustic ammonia produces a yellow precipitate, but becomes blue itself from copper. The residue comports itself with salt of phosphorus like pure oxide of uranium.

Johannite appears, therefore, to contain sulphuric acid, water, and the oxides of copper and uranium. We expect to hear even of the exact ratio of these ingredients from Professor Berzelius, to whom Mr Selfström was kind enough to take a specimen from me.

This species is as rare as it is beautiful. The only specimens hitherto known were found in opening some old works near the mine of Elias at Joachimsthal, in Bohemia, in the year 1819, as a coating of fragments of uranium-ore.

Free sulphuric acid, as is likewise supposed by John, probably owing to the decomposition of some species of pyrites, is no doubt the cause of the formation of the present species. In the specimens which I examined it is accompanied by acicular crystals of gypsum.

Art. XV.—*Notice of a mass of Meteoric Iron recently discovered in Bohemia.*[*]

The locality where this mass of meteoric iron was found, is the slope of a hill near the castle of Bohumilitz, in the circle of Prachin in Bohemia, the estate of Baron Malowetz of Skalitz. A ploughman, having on the 19th September 1829, accidentally alighted upon it with his plough, and supposing the mass, which was afterwards found to weigh 103 pounds, to be an ordinary stone, he endeavoured to lift it, and throw it out, but being surprised with the great weight, he thought it must be a precious metal. A small bit of it, however, having been detached by a blacksmith with a hammer, it was recognized to be iron. Dr Charles Claudi, an eminent lawyer of Prague, the proprietor of the neighbouring estate of Cykin, paying a visit to the baron, was shown the mass, and as there are no iron-works in the vicinity, he argued that it might have had a meteoric origin. This was fully confirmed by Professor Steinmann's discovery of nickel in it, and by the peculiar structure which is likewise detected in other kinds of meteoric iron by etching a polished surface. Upon the application of these gentlemen, Baron Malowetz presented the whole of this highly remarkable object to the National Museum at Prague.

There can be no doubt that this mass of iron has lain a long time in the soil, the plough having passed over it for ages; and it must be ascribed only to the heavy rains of last summer, that, much soil having been washed away, it came at last within the reach of the plough. Its having been a long time exposed to the agency of air and weather, is also testified by a thick crust of oxide of iron, with which it was covered when first dug out.

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[*] Abstract of several papers in the *Jahrbücher des böhmischen Museums*. No ii. 1830.

No conjectures can be made respecting the age of this mass. There is indeed a notice by MARCUS MARCI DE KRONLAND,* that a metallic mass had fallen from the sky in Bohemia, in the year 1618, but without the locality where it had fallen.

According to the account by Professor Zippe, the Bohumilitz meteoric iron is an irregular lump of a somewhat quadrangular shape. It is marked on the surface with irregular roundish impressions, of the same kind as other masses of native iron, having a meteoric origin. He describes the colour of the surface as clove-brown, with spots of ochre-yellow, owing to the oxidation of the surface, which is covered with a crust of the brown hydrate of the peroxide. Within the colour is paler than the colour of newly filed bar iron, but not so pale as that of the Elbogen native iron.

A polished surface, etched with nitric acid, shows the characteristic damask-like delineations first observed by Widmannstetten. They are, however, slightly different from these in the delicacy and angular disposition of these figures. Those of Elbogen are usually thin and distinctly triangular, meeting at angles of 60° and 120°, whereas in those of Bohumilitz the lines are thicker, and meet at angles not always exactly the same; those of 70° and 110°, however, are more usually found. The whole appears to be a compound of several individuals, which may be distinctly seen, when slices are broken across, or in the fracture of the fragment, first detached by the blacksmith.

Cleavage may be distinctly traced in planes perpendicular to each other, which leads to the hexahedron as the fundamental form. They were obtained by cutting through the mass, but not entirely, and then breaking across the remainder; but, on account of the great toughness of the substance, it is generally interrupted by the hackly fracture.

The mass is traversed by several cracks or fissures, and contains also imbedded nodules of a mixture of plumbago, magnetic iron pyrites, and a white metallic substance, not exactly ascertained. The latter, which occurs likewise in small grains disseminated in the Elbogen meteoric iron, occurs here in larger nodules, particularly in the places where the iron and the plumbago meet. Some of the nodules of the latter are nearly an inch in diameter.

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* Millauer, *Verhandlungen des böhmischen Museums*, 1825. 3.

Professor Steinmann found the specific gravity to be 7.146.

On dissolving the substance in muriatic acid, hydro-sulphuric acid was developed, which being introduced into a solution of acetate of lead, gave a small quantity of a precipitate of sulphuret of lead.

A small residue of 1.12 per cent. of the whole was left, which was insoluble even in nitro-muriatic acid. It proved to be a mixture of plumbago, and of small metallic scales of a steel-grey colour. The solution in muriatic acid being boiled with nitric acid, in order to bring the iron into the state of peroxide, was decomposed by carbonate of potassa, and the precipitate digested with caustic ammonia. The blue ammoniacal solution left gave a residue of 5.11 per cent. of oxide of nickel, by evaporation and subsequent ignition.

The result of the analysis of the Bohumilitz meteoric mass is therefore:

Iron, - - -	94.06
Nickel, - - -	4.01
Plumbago, with another metallic substance not sufficiently ascertained, -	1.12
Sulphur, - -	0.81
	100.00

Art. XVI.—*Remarks on a Natural Rocking-Stone of Granite, surmounted by an ancient cross, illustrative of the early Gaulish Costume; observed near the village of Loubeyrat, in the Province of Auvergne, France.* By S. Hibbert, M. D. F. R. S. E. &c. Communicated by the Author.

Each year the geologist is making the boldest trespasses on the ground which the antiquary, by a sort of prescriptive right, has long claimed. The parallel roads of Glenroy have been celebrated as stupendous monuments of the labour to which our ancestors subjected themselves in preparing artificial hunting grounds for the royal chace of the red-deer, yet they are now more soberly regarded as effected by the natural agencies of ancient lakes. The rocking-stone, which has given rise to divers erudite dissertations upon the state of mechanical knowledge among the Druids, is now more rationally attributed to the concentric disintegration which is induced upon certain rocks by atmospheric causes.* Not many years ago the site of every rocking-stone in Britain was carefully registered as indicating the geographic limits to which the ancient priests of the Celtæ extended their religious institutes; and when the antiquaries of our British Colonies in the East were employed in identifying the ancient religion of the Ganges with that of the Gaulish Ovates, and when lying Bramins amused the credulous philosphers who confided in them, by the legend that the British islands had been known to their priests in their earliest records under the name of "The sacred islands of the West," and that in remote times a college of their order had existed there,—then it was that a groupe of detached rocks, or boulders of millstone grit, situated in the West Riding of Yorkshire, which were remarkable for their grotesque weathered forms, and for the natural rocking-stones which long desquamation had induced, were unhesitatingly concluded, from the indications afforded by their name of the *Bramham* Crags, (that is, for *Bramham* Crags, read *Bramah* Crags,) to be the proud existing monuments of a Braminic college!

It is imposible to allude to such lucubrations, which are still scarcely obsolete, with any degree of seriousness. If conceits like these had gone on, Europe and Asia would have been far too limited fields for Celtic researches. It is remarkable that American geologists have lately described many rocking-stones which exist in the Western World; and if natural objects like these are doomed to be forced into the speculations of our Jonathan Oldbucks, the next disquisition to be expected is one that would assign to the Druids a priority over Columbus, in the discovery and colonization of transatlantic regions.

But if a sneer has been deservedly excited when weathered rocks, such as the Logan-stone and Cheesewring of Cornwall, have entered into mythological discussions, it has been less due to the occasion than to the distorted manner in which such objects have been contemplated. No one can take the most superficial glance at the religious rites of our Celtic and Teutonic ancestors without being convinced, that rock worship prevailed among them to a considerable extent; and, amidst the varied forms which rocks assume, the imagination would fix upon some of them as resembling the gods of their peculiar belief. These would be their Simulacra mœsta deorum, to which frequent allusions are made in the Northern Sagas and elsewhere. And thus, when a superstitious fancy has been enabled to trace certain human lineaments in the outlines of a large boulder, it would become a symbol of one of the deities of the Edda. The Laplanders, a different race, who eventually adopted the religious tenets of the Norwegians, were in the habit, according to Scheffer, of converting any odd-shaped log of wood that struck their imagination in a similar manner into an idol of The Thunderer. No wonder then that such assemblages of rocks as shewed singular diversities in form from the effects of weathering should be particularly consecrated. And during the prevalence of rock worship there is reason to suppose that the ancient Gauls set apart such sites for the celebration of Druidic mysteries, and that here they erected the cromlech or sacrificial stone. In countries, therefore, occupied by this people, however widely they might have been separated, we ought to find indications of the similar religious uses to which rocks were submitted, by the similar artificial forms which they were made to assume. Nor shall we be disappointed. If the cromlech is to be seen in many parishes of Wales, it is likewise to be detected in many arrondissements of the ancient Gaulish province of Auvergne.

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* See Dr MacCulloch's excellent *Dissertation on the Granite Tors of Cornwall.*

But still the question arises, what direct proof have we that the rocking-stone had any connection with the religious belief or veneration of the ancient inhabitants of Gaul? This answer is not so easily made. The equipoised massive stone is alluded to by Pliny, though merely regarded by him as a natural curiosity; and an indecisive reference is made to it in The Argonautics of Apollonius Rhodius;—and this is all. In the absence, then, of a direct proof of its religious use or application, I have long conceived that if it had been really held in reverence by the early inhabitants of Europe, the same notice would have been taken of it in a remote Christian period, as of the open temple which was formed by circular ranges of upright stones. The missionaries who first preached the Gospel in Britain were aware that the task of conversion would not be suddenly effected; and hence it was a proper recommendation of St Augustine that a temporizing system should be adopted. Wherever, therefore, a pagan fane existed, no attempt was made to abruptly destroy it, but, in order to gradually wean the natives from idolatry, permission was obtained that a Christian church should be erected in its vicinity. This accompaniment has been accordingly so often noticed, that it is familiar to every antiquary. Another question, however, now arises, whence is it that no similar association has been noticed in certain other forms of rocks, natural or artificial, which have been considered instrumental to pagan worship? It would, for instance, be a strong indication of the religious use to which the rocking-stone might have been applied, were it either found in contiguity or junction with a Christian cross. That no such association should have been discovered in England can create little surprise, when we consider what efforts were made by the early reformers, and afterwards much more so by the puritans, to destroy all relics of the cross, which were regarded by them as so many genuine marks of The Beast. Among the mountains of Auvergne, however, where similar havoc has not been made, the search after such an accompaniment will be found more successful. In this country, where the natives, from their peculiar dark complexion, show decisive marks of a Celtic origin; where the monuments of antiquity which exist resemble those of Wales and Cornwall, I have at length found the rocking-stone surmounted by the Christian cross. Its site is a tolerably high ground, near, as far as I can estimate, to the village of Loubeyrat. A drawing of it is annexed. (See Plate II. Fig. 11.)

This rocking-stone, which is composed of granite, is not very considerable. Its dimensions are from two to three and a-half feet broad by twenty inches in height. It is nicely poised upon another stone of granite; but in order to prevent it from rocking after the cross had been superimposed, its steadiness has been secured by several rude blocks of stones, which are jammed into the interval round its base of support. These are *not* represented in the drawing. On the top of the rocking-stone a square niche has been let in for the reception of a pedestal destined to the support of the cross. This pedestal is two feet one inch in height; its base is twenty-two inches square, from which it gradually lessens to the summit, which is only fifteen inches square. On one side of the pedestal two figures are sculptured, which from their dress appear of great antiquity. The garb is like that of certain figures which are to be found on the capitol of the more ancient part of the Church of Volvic in Auvergne, which is attributed to so early a date as the sixth century, but was certainly not later than the eighth or ninth. But the speculations connected with this altar, I shall separately consider. An inscription appears under the figures, which, from being much worn, is become unintelligible. I could only make out the word PARDON; the remaining letters probably alluded to the number of days of pardon which this cross gave to the venerator. The cross, however, which was contemporary with the pedestal, has been removed, as the one by which it has been replaced, though ancient, is evidently of later workmanship. It has been wrought from the black lava of the country, and has a height from the pedestal of about two feet.

From the tenor of the foregoing observations it is evident, that if a doubt be placed upon the legitimate authority of the antiquary to warn the geologist off his manor when he assigns the formation of the rocking-stone to the disintegrating effect of atmospheric causes, the geologist, on the other hand, has no right, though with much older pretensions of title, to include within his own pale the subject of contention, and to make it a part and parcel of his own demesnes.

Regarding, however, the particular use to which rocking-stones were applied, notwithstanding they have been the subject of infinite learned memoirs, we are still most imperfectly informed; nor is it at all probable that the obscurity will ever be removed. As they are the products of every country where loose detached rocks of a particular structure have been submitted to the operation of atmospheric agents, it is to be expected that the fables assigned to their origin would be regulated by the peculiar mythology of the people among whom they have become the object of notice and wonder. In Greece two standing stones, one of which was a rocking-stone, have been attributed to Hercules, who is said to have reared them in memory of the two sons of Boreas, whom he slew in the sea-girt Tenos

"And one still moves, how marvellous the tale!
With every motion of the northern gale.
FAWKES's *Apollonius*, Book i. line 1676, &c.

Borlase, whose uncontrolled fancy has coined more antiquarian legends than almost any other man, stoutly maintains from this passage that it was *quite usual* with the ancients to place, for a religious memorial, one vast stone upon another so equally poised that the least external force, even a breath of wind, could cause a vibration. This is, to say the least of it, a most illogical inference, as the very circumstance of the vibrating property of such immense stones having been attributed to the supernatural strength of Hercules, ought to have afforded an argument for their comparative rarity.

In the British islands also, where rocking-stones are far more frequent than in Greece, and where it is the common voice that they were the work of those everlasting rock-pilers the Druids, these natural objects have afforded themes for the most amusing conjectures, as that when violently pushed and reverberating, they were suited to alarm the country upon the approach of an enemy;—or that they were used for divination, the vibrations serving, with the assistance of a little secret priest-juggling, for the determination of national questions; or, as an ordeal, for judiciary purposes;—and so plausible, indeed, was this last fable, which was invented by Toland, that it has been made to form a very striking incident in Mason's classical drama of Caractacus:

——————————— Thither youths
Turn your astonish'd eyes; behold yon huge

> And unhewn sphere of living adamant,
> Which, pois'd by magic, rests its central weight
> On yonder pointed rock: firm as it seems,
> Such is its strange and virtuous property,
> It moves obsequious to the slightest touch
> Of him whose breast is pure: but to a traitor,
> Tho' ev'n a giant's prowess nerv'd his arm,
> It stands as fix'd as Snowdon. No reply;
> The Gods command that one of you must now
> Approach and try it!

But to return to the specimen of this kind at Auvergne.—I have little more to observe regarding it, except that the province in which it occurs is remarkable for the indubitable memorials of rock worship or reverence, to which in Britain we give the name of Cromlechs. In fact, no portion of ancient Gaul can perhaps boast of a greater number of them, and hence, the costume of the figures represented on the surmounting pedestal of the cross becomes somewhat interesting, as illustrating the ancient attire of the Gael. It has been long since remarked by Shaw in his *History of Morayshire*, that when Sidonius Apollinaris, a bishop of Auvergne, was in the fifth century particularizing the dress of "a Gothic Gentleman" of his country, he was actually describing the costume of the Scottish Highlands. "He covers his feet to the ankle with hairy leather or rullions; his knees and legs are bare; his garment is short, close and party coloured, hardly reaching to his hams; his sword hangs down from his shoulder, and his buckler covers his left side." It must be confessed, however, that few of these characteristics can be traced in the costume of the two figures rudely sculptured upon the ancient stone of Auvergne, with the exception of what may be fairly implied to be a kilt, exactly like that which the Highlanders wear at the present day, and which, as Apollinaris states, scarcely reaches to the hams. But in discussing the antiquity of the Highland kilt, I am aware that I am entering upon debateable ground, and shall therefore abruptly bring this memoir to a close:

> Non nostrum inter vos tantas componere lites.

ART. XVII.—*On Pyrophosphoric Acid and its Salts.*[*] By Professor STROMEYER of Gottingen.

PROFESSOR STROMEYER remarks, that one of his pupils noticed, several years ago, that phosphate of soda, after being heated to redness, yields a white instead of a yellow precipitate with nitrate of silver. This observation was confirmed by the Professor at the time; and he likewise found, as Gay-Lussac has done lately, that the acid prepared by the action of nitric acid on phosphorus, after exposure to a red heat, precipitated silver in the same manner. The product of the combustion of phosphorus in air or oxygen gas was observed to possess the same property; but the subject had not further occupied his attention until the essay of Mr Clarke, published in the former series of this *Journal*, (vol. vii. p. 298,) induced him to renew the inquiry with the view of explaining the nature of the change.

Stromeyer confirms the statement of Clarke, that the conversion of common phosphate into pyrophospate of soda is attended with no other change than the escape of a little water. He rightly argues that the change cannot in any respect be similar to that which a red heat occasions in sulphite of soda, which, without loss or gain in weight, is converted into sulphate of soda and sulphuret of sodium, since phosphoric becomes pyrophosphoric acid when heated alone, as well as when combined with an alkaline base. In fact, Stromeyer's researches satisfactorily prove the two acids to be essentially distinct, though in the ratio of their elements they appear to be identical.

One obvious distinction between these acids is in the difference of the salts which they form with oxide of silver. These compounds differ not only in colour, but in specific gravity. The density of the pyrophosphate is 5.306, while that of phosphate of silver is 7.321; and hence, in precipitating equal quantities of silver by pyrophosphate and phosphate of soda, the precipitate occasioned by the former is much more bulky than that produced by the latter. They are both pulverulent when dry, and in that state are anhydrous; but the pyrophosphate at the moment of precipitation, retains a little combined water, though, as in the case of carbonate of lime, the union soon spontaneously ceases.

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[*] Abstract of an essay read at the Royal Society of Gottingen in January 1830.

Pyrophosphate of silver fuses with extreme facility, even at a temperature below that of redness, yielding a dark brown coloured liquid, which, without suffering the least appreciable decomposition, becomes a radiated crystalline mass on cooling. By the first impression of heat, long before fusion, it changes to brownish yellow, and retains when cold a shade of the same colour. Phosphate of silver, on the contrary, is very infusible; so that it may be heated in a glass tube, or on platinum foil, to full redness, without being fused. Its colour changes to a reddish-brown by very slight elevation of temperature; but, as it cools, the original yellow tint is restored. At a white heat it is fused; and then, like the pyrophosphate, it assumes a dark brown colour, but regains the yellow on cooling. If kept for any time in a fused state, a portion of pyrophosphate is generated; and, consequently, the mass becomes more fusible, and acquires a paler tint. A very minute quantity of pyrophosphate greatly increases the fusibility of the phosphate of silver.

Phosphate of silver is blackened by exposure to light, while the pyrophosphate receives a reddish tint. They are both insoluble in water, and unchanged by boiling. The pyrophosphate, like the phosphate, is easily dissolved by nitric acid, and is thrown again by ammonia; but if heated with nitric acid, ammonia then precipitates the yellow phosphate. By muriatic acid it is instantly decomposed, with production of chloride of silver, and separation of pyrophosphoric acid. Sulphuric acid acts like the nitric; but no change is produced by acetic acid. It is readily dissolved by means of ammonia, and falls wholly unchanged, when the solution is neutralized.

Phosphate of silver and pyrophosphate of soda may be boiled together without the least change; but double decomposition is instantly produced by boiling pyrophosphate of silver with phosphate of soda, the insoluble salt changing from white to yellow. Other insoluble pyro-phosphates, such as pyrophosphate of lead, copper, and zinc, undergo a similar decomposition when boiled with phosphate of soda. Consistently with these facts it is found, that when nitrate of silver is gradually added to a mixture of phosphate and pyrophosphate of soda, the first precipitate consists of phosphate, and the latter of pyrophosphate of silver. It is hence inferred by Professor Stromeyer, that in the intensity of its attraction for salifiable bases phosphoric acid exceeds the pyrophosphoric, —a circumstance which alone suffices to establish an essential distinction between these compounds.

The insoluble pyrophosphates are in general easily dissolved in a solution of pyrophosphate of soda. This has been observed with the pyrophosphates of silver, lead, copper, nickel, cobalt, uranium, bismuth, manganese, protoxide of mercury, glucina, and yttria; and, therefore, in preparing these salts by way of double decomposition, it is important to avoid an excess of the precipitant. To this remark the pyrophosphates of peroxide of mercury, oxide of chromium, baryta, strontia, and lime, are exceptions; but even the three latter, when recently precipitated, are dissolved in a small degree by pyrophosphate of soda. The circumstance here adverted to, appears to depend on the formation of double salts which are very soluble in water. It forms a striking contrast with the phosphates; for the insoluble compounds formed by double decomposition with phosphate of soda, are, almost without exception, quite insoluble in excess of the precipitant.

Stromeyer concludes, from the preceding facts, that pyrophosphoric acid is essentially distinct in chemical constitution from the phosphoric; and that it is equally well entitled as phosphorous and hypophosphorous acids to be regarded as a distinct acid of phosphorus. But the nature of the difference, whether arising from a different stage of oxidation, or from any other cause, does not appear, and requires for its elucidation the aid of additional experiments.

As the pyrophosphoric acid obtained by heating phosphoric acid to redness, as well as that which is formed by the combustion of phosphorus, loses, by exposure to the air, its property of giving a white precipitate with nitrate of silver, and then resembles in every respect common phosphoric acid, it appeared probable that the conversion of the latter into the former was owing to deoxidation, and that pyrophosphoric may be regarded as a hypophosphoric acid, intermediate between phosphoric and phosphorous acids. It cannot, however, be a variety of phosphatic acid, since it does not reduce the salts of mercury, nor is it inflamed by a strong heat; but the idea of its being less highly oxidized than phosphoric acid, is, nevertheless, supported by the analogy of phosphorus with sulphur, selenium, and arsenic, which do not pass into a maximum of oxidation when burned, and when fully oxidized are easily deprived of some of their oxygen. Another circumstance, favourable to the same line of argument, is, that pyrophosphoric acid and pyrophosphate of soda, when heated with nitric acid, are speedily converted into phosphoric acid and phosphate of soda. Against this opinion, however, the following facts are decisive:—that during the change just mentioned, the nitric acid does not appear to undergo the least trace of decomposition; that a similar conversion is effected not only by sulphuric, muriatic, and acetic acids, but even by the phosphoric; that free pyrophosphoric is converted into phosphoric acid by mere boiling in water, without this fluid suffering the slightest decomposition; and lastly, by the change of phosphoric acid and phosphate of soda into pyrophosphoric acid and pyrophosphate of soda being unattended by any evolution of oxygen gas.

Nor can it be maintained that the pyrophosphoric is an oxygenized phosphoric acid. Phosphate of soda, when heated to redness, does not absorb any oxygen from the atmosphere, nor is any hydrogen or phosphorus evolved: the salt, in fact, loses nothing, as Clarke observed, but a small quantity of water. Nor does the difference between phosphate and pyrophosphate of silver depend on the presence of water in one of them, since they are both anhydrous; and the facts above-mentioned, establishing a decided contrast between the phosphates and pyrophosphates in general, sufficiently prove that the phenomena cannot be owing to a mere difference in the ratio of acid and base. Stromeyer is therefore disposed to consider the two acids as differing, in chemical constitution, rather in the mode in which their elements are arranged, than in the proportion in which they are united.

In order to determine this point with certainty it was necessary to make a comparative analysis of a phosphate and pyrophosphate containing the same base, and carefully to investigate, not only the composition of the pyrophosphate, but the phenomena attending its conversion into the corresponding phosphate. The pyrophosphate of silver was selected for this purpose.

As a mean of two experiments, Stromeyer found that 100 parts of pyrophosphate of soda, when decomposed by nitrate of silver, yield 222.085 parts of pyrophosphate of silver. Known weights of fused nitrate of silver were then precipitated by phosphate and pyrophosphate of soda; and as the mean of two experiments with each, and estimating 100 parts of the fused nitrate to contain 68.6 of oxide of silver, it was found that 118 parts of oxide of silver unite with 38.36 of pyrophosphoric acid, and with 22.96 of phosphoric acid.

The composition of these salts was also ascertained by dissolving each in dilute nitric acid, and precipitating the silver, in some instances with muriatic acid, and in others by sulphuretted hydrogen. The results closely corresponded with each other, and with those above stated. As a mean of all the analyses, it follows that phosphate and pyrophosphate of silver are thus constituted:

	Phosphate of silver.		Pyrophosphate of silver.	
Oxide of silver,	83.455	118	75.39	118
Acid, -	16.545	23.394	24.61	38 52
	100.000	141.394	100.00	156.52

The relative quantity of the two acids, united with the same quantity of oxide of silver, is in the ratio of 23.394 to 38.52, or as 3 to 5. To this great capacity of phosphoric acid is to be ascribed the acid re-action of a solution in which nitrate of silver has been decomposed by phosphate of soda; a circumstance by no means confined to nitrate of silver, but which occurs with salt containing a strong alkaline base, such as lime and baryta, and is the cause of some of the precipitate being retained in solution. For the same reason, when phosphate of soda is exactly neutralized by phosphoric acid, and is converted by heat into pyrophosphate, the resulting salt is strongly alkaline.

It yet remained to ascertain how much phosphate of silver would be obtained from a given weight of pyrophosphate of soda and pyrophosphate of silver, when the acids of these salts is changed into phosphoric acid. For this purpose, 1.077 grammes of pyrophosphate of soda was converted into a phosphate, by being boiled with nitric acid, and after neutralizing with soda, the phosphoric acid was precipitated by nitrate of silver. The resulting phosphate of silver weighed 3.40 grammes. Stromeyer then dissolved 1.712 grammes of pyrophosphate of silver in dilute nitric acid,—threw down the silver by sulphuretted hydrogen,—converted the pyrophosphoric into phosphoric acid, and by means of nitrate of silver, employed as in the preceding analysis, he obtained 2.543 grammes of phosphate of silver. This last experiment was repeated with 2.336 grammes of pyrophosphate of silver, which yielded 3.350 grammes of phosphate of silver.

These experiments, accordingly, afford decisive evidence, that, during the conversion of pyrophosphoric into phosphoric acid, as also in the reconversion of the latter into the former, the weight of the acids do not suffer the least change. They confirm, therefore, the supposition above-mentioned, that these acids are completely identical in the ratio of phosphorus and oxygen, and that the essential differences between them are referable solely to the manner in which their elements are combined. We obtain then in these acids, a new and decisive proof of the fact, that the same elements, though united in the same proportion, may give rise to compounds which differ essentially in their physical and chemical properties; and that, consequently, changes in the constitution of many substances may take place, without any accompanying change in the relative weight of its ingredients. This fact, which in its consequences is of the greatest importance, lays a wholly new field of most interesting chemical inquiry, by which many phenomena, not previously accounted for, will receive a satisfactory explanation. From this source we may anticipate very important results, illustrative of the chemical nature of organic substances; and be thus enabled to perceive how it is possible for so small a number of elements to give rise to such numerous and diversified combinations.

It will readily be anticipated that the observation of Dr Engelhart, " that a solution of albumen is precipitated by recently ignited phosphoric acid, while no turbidity is occasioned by phosphoric acid which has not been heated, depends on the formation of pyrophosphoric acid. This fact has been already stated by Gay-Lussac, (*Annales de Chimie et de Physique*, xli. 332,) and is confirmed by Stromeyer. Albuminous solutions will hence afford an easy mode of distinguishing pyrophosphoric from phosphoric acid.

ART. XVIII.—*Notice regarding the nature of a peculiar Structure observed in the Second Stomach of certain Cetacea, generally considered as simply glandular, but seemingly analogous to the Electric Organs of the Torpedo and Gymnotus.* Communicated in a Letter to the Editor from Dr KNOX. *To which is annexed the Microscopical Examination of the Structure by Dr BREWSTER.*

DEAR SIR, NEWINGTON PLACE, *June* 1830.

AN investigation which I made some time ago into the dentition of the Dugong and Cetacea, led me to re-examine several points in the anatomy of these curious animals, and amongst other structures then brought before me, that of the second stomach in the small species of porpoise caught so frequently in the adjoining Frith and German Ocean, particularly attracted my attention. I regret, that, at the present moment, I have not leisure so to arrange my notes of these dissections, as to fit them for publication in the Journal conducted by you, and that, owing to this, I shall be forced to limit myself to a very brief notice.

In accordance with the language of all anatomists, I shall speak of this species of the Cetacea as having four stomachs, this being the usual way of describing complex stomachs; as those of the ox, sheep, deer, antelope, camel, and whale.

My own opinion, as explained more fully to the Royal Society of Edinburgh, in a Memoir I had the honour to submit to them during the course of last Winter Session, is, that no animal possesses more than one stomach, divided more or less by compartments, and thus assuming the appearance of one or more cavities, which anatomists have unhappily designated as one or more stomachs. Now, in accordance with this language, which, however inaccurate, by reason of its universality requires being respected, I shall speak of the second cavity in the stomach of the porpoise as being the second stomach.

The gullet of the porpoise, composed of the usual membranes or tunics common to it with others of the mammalia, terminates in a somewhat elongated and tolerably capacious pyramidal-shaped bag, to which we shall give the name of first stomach. In this we find externally, and immediately invested by the peritoneal tunic, a strong muscular coat of fibres spread uniformly over the surface, continuous upwards with the muscular layers of the gullet, and downwards with those which, in a similar fashion, envelope the second stomach, occupying the same situation relative to the peritoneal tunic in it as in the first. The muscular tunic of the first stomach is seemingly composed of two distinct layers, separated from each other by a layer of cellular membrane, and the fibres are chiefly longitudinal and circular. Within these two there is the usual celluloso-vascular layer, and it has within it a mucous membrane, covered by a strong epidermic covering. On maceration, we can separate from the mucous surface of the gullet a double epidermic covering, but one only seems to invest the first stomach.

To this cavity the branches of the nervi vagi (which comparatively are very large and distinct,) do not proceed in any great abundance; their course is rather towards the second stomach, whose structure I shall now endeavour to describe.

The capacity of the second stomach or cavity is less than that of the first, and its structure is altogether and most remarkably different from it. The communication is by an aperture admitting the fore-finger, and here the textures of the first stomach suddenly cease. The epidermic covering and subjacent mucous membrane cease, and there is substituted for them a perfectly smooth membrane, without villosities or glandular structures: it has a good deal the appearance of a serous membrane. This closely invests a series of fibres, which externally are covered in by an extremely vascular and cellular tunic. These fibres are not muscular, and have no resemblance to any glandular structure, excepting perhaps the tubular part of the kidney. They are placed perpendicularly, and close to each other betwixt the two membranes I have spoken of; they stand out, therefore, everywhere from the external surface of the inner membrane of this stomach like a pile of velvet enclosed by thin laminæ or plates. Outside the celluloso-vascular lamina muscular layers exist, continued from those of the first stomach, and transmitted over the second to the third. The interior of this second cavity, when laid open, presents a series of longitudinal and transverse elevations, which bear a considerable resemblance to the interlocking of the fingers in each other. To this stomach the greater part of the nervi vagi are distributed.

The third and fourth stomachs or stomachal cavities, have been very carefully described by the Baron Cuvier, and by all systematic writers on comparative anatomy. The question raised by Camper, as to the number of the stomachs in this animal, does not merit particular notice. The accompanying rude design will perhaps explain to the non-professional reader the appearances I have endeavoured to describe. I forbear for the present all speculations as to the nature of these organs. That they are not muscular is to me evident; but future observation and experiment can alone determine whether or not I am correct in supposing them analogous to electric organs, that is, fitted to exercise electric phenomena as connected with the digestion of the food.

Since writing the above, the structure in question has been examined microscopically by Dr Brewster. The result of his examination is as follows:

"I have examined the piece of stomach you have sent me of one of the Cetacea. It seems in its wet state to consist of tubes or fibres perpendicular to the two membranes which enclose them, thus, and the upper surface of one of the membranes is covered with hollows or depressions corresponding with the extremities of the tubes or fibres. A more minute examination, conducted in a different way, proves these perpendicular portions to be tubes. In order to dry the substance I pressed it between folds of bibulous paper, and the consequence of the compression was, to press together nearly all the tubes, and make the whole one dense mass of a dark brown colour; but when it became dry and slightly indurated, I drew it out as if it had been India rubber, and the tubes opened and the mass became *white*, in consequence of the reflexion of the light from the separated fibres. The whole of it consists of fine fibres interlaced, having a remote resemblance to the pith of certain plants.

ART. XIX.—*On the production of regular double refraction in the Molecules of Bodies by simple pressure; with observations on the origin of the doubly refracting structure.* By DAVID BREWSTER, LL. D. F. R. S. L. & E.[*]

IN various papers already printed in the *Philosophical Transactions*, I have had occasion to show that the phenomena of double refraction may be produced artificially by certain changes in the mechanical condition of hard and soft solids.[†] In all these cases the phenomena are related to the form of the mass in which the change is induced; and in the case of hard and elastic solids, they vary with any variation of form which alters the mechanical state of the particles. In isinglass and other bodies to which double refraction has been communicated by induration, the particles take a permanent position, which is not altered by any change of shape; but still the phenomena exhibited by a given portion of the mass are related to the surfaces where the indurating cause operated, and also to those by which the isinglass was bounded; and they depend on the position which that portion occupies in the general mass.

In all these cases the phenomena are entirely different from those of regular crystals, and in none of them is the doubly refracting force a function of the angle which the incident ray forms with one or more axes given in position.

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[*] From the *Phil. Trans.* 1830.

[†] *Phil. Trans.* 1814; 1815, pp. 1, 30, 60; 1816, pp. 46, 56.

As long ago as 1814 I communicated to the Royal Society the following experiment on the depolarizing structure of white wax and resin:

"When resin is mixed with an equal part of white wax, and is pressed between two plates of glass by the heat of the hand, the film is almost perfectly transparent by transmitted light, though of a milky white appearance by reflected light. It has not the property of depolarization when the light is incident vertically; but it possesses it in a very perfect manner at an oblique incidence, and exhibits the segments of coloured rings."*

The subject of double refraction was then so little developed that this experiment excited no notice; and it was only brought to my own recollection by the accidental appearance of the specimen itself. This depolarizing film has suffered no change by remaining fifteen years between the plates of glass. The vertical line along which it is destitute of the property of depolarization is a single axis of double refraction; and the coloured rings at oblique incidences are produced by the inclination of the refracted ray to the axis of double refraction. In order to examine this remarkable effect under a more general aspect, I made a considerable number of such plates with different kinds of wax, and with various proportions of resin, and I was led to results which seem to possess considerable interest.

When the white wax is melted alone and cooled between two plates of glass, it consists of a number of minute particles, each possessing double refraction, but having their axes turned in all possible directions. If the film of wax is made extremely thin, the particles are not sufficiently numerous to exhibit any action upon polarized light.

When resin alone is melted and cooled in a similar manner, it exhibits no doubly refracting structure, whether it indurates slowly or under the influence of pressure.

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* *Phil. Trans.* 1815, pp. 31, 32.

If resin and white wax are mixed in nearly equal proportions, the compound possesses considerable tenacity. When a proportion of it is melted and cooled between two plates of glass, it shows the quaquaversus polarization of bees'-wax, the axes of the elementary particles being turned in every direction. It possesses a considerable degree of opalescence, and a luminous body seen through it is surrounded with nebulous light. This imperfect transparency evidently arises from the reflexion and refraction of the rays in passing from one molecule to another, occasioned by a difference in the refractive power of the ingredients, or by the imperfect contact of the particles, or by both these causes combined.

In order to observe the modifications which these phenomena received from pressure, I took a few drops of the melted compound and placed them in succession on a plate of thick glass, so as to form a large drop. Before it was cold, I laid above the drop a circular piece of glass about two-thirds of an inch in diameter, and by a strong vertical pressure on the centre of the piece of glass, I squeezed out the drop into a thin plate. This plate was now almost perfectly transparent, as if the pressure had brought the particles of the substance into optical contact.

If we expose this plate to polarized light, we shall find that it possesses one axis of positive double refraction, and exhibits the polarized tints as perfectly as many crystals of the mineral kingdom. The structure thus communicated to the soft film by pressure does not belong to it as a whole, nor has it only one axis passing through its centre like a circular piece of unannealed glass. In every point of it there is an axis of double refraction perpendicular to the film, and the doubly refracting force varies with the inclination of the incident ray to this axis, as in all regular uniaxal crystals. When the two plates of glass are drawn asunder, we can remove one or more portions of the compressed film, and these portions act upon light exactly like films of uniaxal mica or hydrate of magnesia, and develope a doubly refracting force of equal intensity.

This remarkable experiment presents an interesting subject of inquiry. That the regular double refraction of the film is developed by the agency of pressure cannot be doubted; but it does not at first sight appear whether it is the immediate effect of the pressure, or is the same doubly refracting force which produces the quaquaversus polarization that takes place when the resinous film indurates without constraint. In this state of the film the axes of double refraction are clearly turned in every conceivable direction; and it is impossible to suppose that a pressure in one direction could suddenly arrange all these axes in parallel positions. The double refraction of each particle of the film has therefore been developed by the compressing force similarly applied to them; and in producing this effect, it must have deprived each particle of the doubly refracting structure which it previously possessed. The substitution of one doubly refracting structure for another may be easily effected in many bodies. Even in regular crystals we can by heat or pressure modify or remove their double refraction. Nay, we can take away one axis from a biaxal crystal, and communicate a second axis to an uniaxal one. When the doubly refracting structure is produced by induration, we can remove it wholly by pressure, and replace it with another even of an opposite character; and when it is generated by the living principle, as in the case of the crystalline lenses of animals, we can take it away entirely, and substitute a new and more powerful doubly refracting structure by induration.

We may therefore consider it as clearly established that the uniaxal double refraction of the resinous mass has been communicated to the individual molecules by simple pressure; the increased transparency arising from the molecules being brought into closer contact, and the regular double refraction from the variable density impressed upon each elastic molecule, and symmetrically related to the axis of pressure. The effect thus produced on the resinous mass is precisely the same as what would take place by subjecting elastic spheres to a regular compressing force. The axis of pressure becomes an axis of positive double refraction, and the double refraction increases with the inclination of the ray to the axis, and becomes a maximum in the equator of the molecules.

By this view of the preceding facts, we are led to a very simple explanation of the origin and general phenomena of double refraction in regular crystals. That this property is not inherent in the molecules themselves may be easily proved. The particles of silex, for example, do not possess it in their separate state. In tabasheer, in many opals, and in melted quartz, there is not the slightest trace of the doubly refracting structure: but when the particles of silex in solution are allowed to combine, in virtue of their polarities or mutual affinities, they then instantly acquire, at the moment of their combination, the property of double refraction, and they retain it while they continue in this state of aggregation. The manner in which this takes place may be easily conceived: a number of elastic molecules existing in a state of solution, or in a state of fusion, are kept at such a distance by the fluid in the one case, and by the heat in the other, as to preclude the operation of their mutual affinities; but when, in the process of evaporation or cooling, any two molecules are brought together by the forces or polarities which produce a crystalline arrangement, and strongly adhere, they will mutually compress one another, and each will have an axis of double refraction in the directions of the line joining their centres, in the same manner as if they had been compressed by an external force.

From the phenomena of crystallization and cleavage, it is obvious that the molecules of crystals have several axes of attraction, or lines along which they are most powerfully attracted, and in the direction of which they cohere with different degrees of force. Guided by the indications of hemitrope forms, and supposing the molecules to be spherical or spheroidal, we infer that their axes are three in number and at right angles to each other, and are related in position to the geometrical axis of the primitive form. In like manner the phenomena of double refraction are related to the same axis of the primitive form, and may be all rigorously calculated by a reference to three rectangular axes. In uniaxal crystals, the three axes A, B, C must be such that two of them are equal and of the same name; while the third, corresponding with the apparent axis, may be of the same or of a different name. In biaxal crystals, the three axes A, B, C are unequal, and in crystals with no double refraction the axes are equal and destroy each other.*

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* In uniaxal crystals, the resultant of the two equal axes A, B may

This approximation of these two classes of facts is too remarkable to be accidental, and would go far to establish their dependence, even if it were not indicated by other arguments which I shall proceed to illustrate.

Among those crystals which have the obtuse rhomboid for their primitive form, there are many with one axis of negative double refraction, and only one or two with one axis of positive double refraction. In the former, the negative doubly refracting structure will be produced round the axis of the rhombohedron by the compression arising from attractions in the direction of two equal rectangular axes A, B, which will dilate the molecules in the direction of the third axis C, and make it a negative axis of double refraction, equal in intensity to either of the other two. Here we require the combination only of two axes; but if we suppose that there is in the direction of C a third axis of attraction either more or less powerful than the other two, then if it is less powerful, the compression of the molecules produced by it will diminish the dilatation arising from the united action of A and B, but will still leave an unbalanced dilatation, or a single negative axis of double refraction in the axis of the rhomb.

If C, on the contrary, is an axis in which the attractive force of the molecules is greater than along A and B, the compression which it produces will exceed the dilatation arising from A and B, and we shall have an axis of compression along C, or an axis of positive double refraction as in quartz and dioptase*. The same observations are applicable to minerals that crystallize in the pyramidal form.

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have any relation to C but that of equality; excepting when C is of a different name from A and B.

In biaxal crystals, any two axes A, B, may be converted into three A + C, B ± C, ± C. See *Phil. Trans.* 1818.

* Since this paper was written, I have seen the very valuable researches of M. Savart on the structure of crystallized bodies as developed by sonorous vibrations. The curious result of his experiments, that the axis of calcareous spar, a negative axis of double refraction, is the axis of least elasticity, while the axis of quartz, an axis of positive double refraction, is the axis of greatest elasticity, harmonizes in a remarkable manner with the above views.

When the three axes A, B, C are all equal, the three rectangular compressions, produced by the aggregation of the molecules, will destroy one another at every point of the molecule, and the body which they compose will have no double refraction, and cleavages of equal facility. Hence all crystals in which it is known by cleavage that the particles cohere with equal force in three rectangular directions have actually no double refraction.

If the three attractive axes A, B, C are all unequal, the difference of density which they produce in the molecules will be related to two axes of double refraction, the strongest of which will be negative or positive according as the compression along C is less or greater than the dilatation produced along C by the united compressions of A and B. Hence all crystals belonging to the prismatic system, in which we are informed by cleavage that the particles cohere with unequal forces in three directions, have invariably two, or, as we have already explained, three unequal axes of double refraction, of which the strongest is sometimes positive and sometimes negative.

We have supposed the elementary molecules of bodies to be spherical when existing singly, or beyond the sphere of their mutual action; but although their form must, in the case of doubly refracting crystals, be changed into oblate, prolate, or compound spheroids, yet the deviation of these spheroids from the sphere may be so small, that the forms of the bodies which they compose may be regarded as arising from the union of spherical molecules. It is more probable, however, that the form of the molecules suffers a considerable change, and we may consider that change as determining the exact primitive form of the crystal and the inclination of its planes.

The circumstance of almost all rhombohedral crystals having negative double refraction, which can only be produced by axes of compression in the equator of a prolate spheriod, excludes the supposition, that the ultimate molecules are spherical particles converted by the forces which unite them into those oblate and prolate spheroids, by means of which, according to the views of Huygens, all the varieties of rhombohedrons may be formed;* for if this were the case, the obtuse rhombohedrons should possess one positive axis, and the acute ones one negative axis of double refraction. We are constrained therefore to suppose that in rhombohedral crystals the molecules have the form of an oblate spheroid, with its axes so related, that the change superinduced upon it by the forces of aggregation determines the exact form of the combination. In carbonate of lime for example, where the precise inclination of the faces of the rhombohedron can be produced only by oblate spheroids whose polar is to their equatorial axis, as 1 to 2.8204, we may suppose that the spheroids were originally more oblate, and that the forces by which they receive the doubly refracting structure dilated them in the direction of the smaller axis, so as to produce a spheroid having its axis as 1 to 2.8204. Hence if we could suppose the molecules placed together without any forces which would alter their form, they would compose a rhombohedron with a greater angle and having no double refraction. But when they are combined by the attractive forces of crystallization, they compose a rhombohedron of 105°, possessing negative double refraction.

In this view of the subject, the form of the ultimate molecules of crystals existing separately, may be regarded as determining, within certain limits, the primitive form to which they belong; while the doubly refracting structure and the precise form of the crystal are simultaneously produced by the action of the forces of aggregation.

These views receive a remarkable illustration from a new doubly refracting structure, which I discovered many years ago in chabasie, and which will form the subject of a separate communication. In certain specimens of this mineral, the molecules compose a regular central crystal, developing the phenomena of regular double refraction; but in consequence of some change in the state of the solution, the molecules not only begin to form a hemitrope crystal on all the sides of the central nucleus, but each successive stratum has an inferior doubly refracting force till it wholly disappears. Beyond this limit it reappears with an opposite character, and gradually increases till the crystal is complete. In this case the relative intensities of the axes or poles from which the forces of aggregation emanate, have been gradually changed, probably by the introduction of some minute matter, which chemical analysis may be unable to detect. If we suppose these axes to be free, and the foreign particles to be introduced, so as to weaken the force of aggregation of the greater axis, then the doubly refracting force will gradually diminish with the intensity of this axis, till it disappears, when the three axes are reduced to equality. By continuing to diminish the force of the third axis, the doubly refracting force will reappear with an opposite character, exactly as it does in the chabasie under consideration.

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* See Huygens's *Traité de la Lumière,* chap. v. and the *Edinburgh Journal of Science,* No. xviii. pp. 311, 314.

From the mutual dependence of the forces of aggregation and double refraction, it is easy to understand the influence which heat produces on the doubly refracting structure, as exhibited in the phenomena discovered by M. Mitscherlich in sulphate of lime and calcareous spar, and in those which I detected in glauberite. * This eminent philosopher has found, by direct experiment, that heat expands a rhomb of calcareous spar in the direction of its axis, and contracts it in directions at right angles to that axis; † that the rhomb thus becomes less obtuse, approaching to the cubical forms which have three equal axes, and that its double refraction diminishes. All these effects are the necessary consequences of the preceding views. The expansion in the direction of the axis, and

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* See *Edinburgh Transactions*, vol. xi.

† It follows from this fact, that massive carbonate of lime, in which the axes of the molecules have every possible direction, should neither expand nor contract by heat, and would therefore form an invariable pendulum. As there must be, in any given length of massive carbonate of lime, as many expanding as there are contracting axes, then, if the contractions and expansions in each individual crystal are equal, they will destroy one another; but if they are proportional to their lengths, the contractions will exceed the dilatations. In this case, we have only to combine the marble with an ordinary expanding substance, to have an invariable pendulum. The balances of chronometers might be thus made of mineral bodies.

the contraction of all the equatorial diameters diminish the compression of the axes of the oblate spheroidal molecules, and must therefore diminish its double refraction, as well as the inclination of the faces of the rhomb. In like manner it will be found that in sulphate of lime and glauberite the expansions and contractions will be so related to the three axes, as to explain the conversion of the biaxal into the uniaxal structure, and the subsequent reappearance of the biaxal structure in a plane at right angles to that in which the axes are found at ordinary temperatures.

The phenomena exhibited by fluids under the influence of heat and pressure, and those of doubly refracting crystals, exposed to compressing or dilating forces, are in perfect conformity with the above views; so that even without the fundamental experiment described in this paper, we might have been entitled to conclude that the forces of double refraction are not resident in the molecules themselves, but are the immediate result of those mechanical forces by which these molecules constitute solid bodies.

ALLERLY, *October 5th*, 1829.

ART. XX.—*Experiments of Candle Wicks.** By Mr JOHN REID, Member of the South African Institution.

THE fat of the sheep or the cow when exposed to the temperature of 120° of Fahrenheit melts, and when exposed to the temperature of 500° suffers decomposition, and is converted into gaseous compounds consisting of carbon, hydrogen, and oxygen. At this elevated temperature they burn in contact with atmospheric air, the combination being attended with the extrication of caloric and light.

When a candle is lighted a portion of tallow is melted, is attracted by the wick till it is brought within reach of the flame, where it is converted into the compounds above-mentioned, which, combining with the oxygen of the atmosphere, give out caloric and light. This caloric melts another portion of tallow, which is attracted, decomposed, and undergoes combustion, and thus is a regular supply kept up.

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* Read at the South African Institution, and taken from the *South African Quarterly Journal*, No. ii. p. 121.

Combustion requires an elevated temperature; if therefore any substance at a low temperature is brought near the flame, it abstracts a portion of caloric, and causes a diminution of its size, and if brought still more close, or a substance at a lower temperature is made to approach it equally near, it is entirely extinguished.

These circumstances serve the important purpose of regulating the combustion, for if this communication of caloric to surrounding objects did not take place, and if the combination was effected at a low temperature, the whole mass would suffer almost instantaneous conflagration. When a candle is lighted or relighted, the wick either having no tallow, or only a small quantity, is soon consumed, and the flame is forced to descend till it comes near the mass of unmelted matter; in this case, or when the wick is snuffed too close, the caloric being abstracted too rapidly, the flame is diminished in size; and on account of its proximity to the mass, melts a greater quantity of tallow than is required for the proper supply, which accumulates and makes the candle gutter, causing waste and inconvenience. On the other hand, when a candle has burnt for some time, the wick becomes too long, diminishing by its presence the quantity of light evolved by the combustion. This obscuration may be partly owing to the shadow of this opaque body, but is in part owing also to the influence it exerts upon the chemical process which takes place. As the wick is not consumed, it requires a constant supply of caloric to keep it at an elevated temperature, which being abstracted from the combustible compounds lowers their temperature, renders the combustion less complete, and produces a yellow flame instead of the usual white flame which diffuses more light. In consequence of this imperfect combustion, a portion of carbon is deposited, which either passes off in the form of smoke, or adheres to the wick increasing its size. It is therefore of importance that the wick should be of a proper length, that it may on the one hand afford sufficient surface upon which combustion may take place, and on the other not diminish more than is necessary the effect of the light which the combustion renders sensible.

The thickness of the wick is of importance as well as its length. When not sufficiently thick it is apt to incline downwards and fall upon the candle; or if it remain upright it does not attract a sufficient supply of melted tallow for the combustion: when it is too thick, though a sufficient quantity of melted tallow may be attracted and consumed, the illuminating effect is diminished in the same way as when it is too long, the diminution being greater in proportion to its size. To remedy this it was suggested to me to try the effect of a flat wick; accordingly I made a candle with a wick consisting of three separate cords placed in a plane with each other, the breadth of which consequently exceeded its thickness; I also made another candle with two wicks placed at a distance from each other, each wick containing five threads; a third with three wicks each, containing five threads; and a fourth with one round wick, containing twenty threads, and compared the illuminating effect of each with that of a common wax candle. The manner in which the experiment was made was the following: Two square boxes were procured, each having one side open, the top and bottom being closed, these were placed with the open sides facing a wall; in one I placed the wax candle, in the other box each of the other candles successively, and between the two boxes a cylindrical object at the distance of four inches from the wall; both candles being lighted and snuffed so as to have the wicks of the length best adapted for giving the maximum of light. Keeping the wax candle at the distance of 18 inches from the wall, the others were moved backwards or forwards according to circumstances, so as to obtain a shadow of equal intensity from each candle. The following are the results which I obtained, but which, considering that the flame of a candle is at all times variable, can only be looked upon as approximations to the average effect. As the number 18 indicates the distance in inches at which the wax candle was placed from the wall, so the number opposite each of the other candles shows the distance at which they severally produced a light equal to that from the wax candle.

Common wax, - - -	18
Tallow with flat wick composed of three cords, each containing 5 threads, - -	17
Tallow with one wick, containing 20 threads,	15
Tallow with two wicks, each containing 5 threads.	18
Tallow with three wicks, each containing 5 threads,	26½

It has been ascertained by experiment, that the luminous effect is increased or diminished in proportion to the square of the distance; therefore, if one body produces the same effect at the distance of sixteen inches as another at twelve inches, the illuminating power is as nine to sixteen. This principle enables us to find out the comparative quantity of light emitted from each of these candles. But for our present purpose it is sufficient to say, that the candle with three wicks, containing in all fifteen threads, produces in burning the same effect at the distance of 26½ inches as that with two wicks, containing in both ten threads at the distance of 18 inches, and the same as that with one wick, containing twenty threads at the distance of 15 inches, and so with regard to the others.

In endeavouring to ascertain the effect produced by diminishing the size of the wick, using as before a wax candle as the standard, I obtained the following results:

Wax, - -	-	-	- -	18
Tallow with three wicks,	each	containing	5 threads,	16½
Ditto,	ditto,	ditto,	4 threads,	$23\frac{5}{4}$
Ditto,	ditto,	ditto,	3 threads,	21½

A circumstance which influences the illuminating effect, is the distance at which the wicks are apart from each other, as it increases till they are at the distance of a quarter of an inch, beyond which when five threads compose the wick, two flames are formed, but does not materially diminish at a distance of one-third of an inch, when the flames are completely separate. The most eligible distance would be a quarter of an inch, but as in burning they sometimes vary their position, approaching to or receding from each other, the distance of one-sixth of an inch is to be preferred, at which we may always obtain one flame from both.

On endeavouring to ascertain the comparative quantity of light evolved from a given quantity of tallow, using a common tallow candle and one with two wicks, I found that when both are kept snuffed closely, there is but little difference; in one experiment the quantity of tallow consumed in half an hour, care being taken during the time to keep the flame in each equal, being of the

Common tallow candle,	-	68 grs.
Candle with two wicks,	-	66 grs.

In another experiment the quantity consumed was the same in each. In another experiment I allowed both candles to burn without snuffing them, till the wick attained the length of an inch, in which state the consumption of tallow is not much different to that which takes place when they are kept of a moderate length. The result I obtained was as follows·

	Inches.
Wax candle as before, - - -	18
Tallow with two wicks, each containing 8 threads, when kept snuffed, - - -	21½
Allowing it to remain unsnuffed, - -	19½
Common tallow candle with one wick, containing 20 threads, when kept snuffed, - -	15
Allowing it to remain unsnuffed, - -	7½

As these observations show that a candle with two wicks gives a light nearly equal to that of a wax candle, I shall endeavour to point out some of those circumstances which require attention in attempting to make it available for useful purposes. When each thread is not twisted separately, but the different threads are twisted together, each wick in burning constantly changes its position, as occurs frequently in a spermaceti or wax candle, where the end of the wick points sometimes in one direction, sometimes in another, therefore the two wicks at one time recede from and at another approach towards each other, or bend on different sides laterally; but when each thread is twisted separately, and the whole are then twisted together, strength is given to the wick, and it remains more steadily in one position.

A certain number of threads is necessary for giving that strength which is required to keep the wick from bending. Though a sufficient degree of light may be obtained from two wicks, each consisting of six or even three or four threads, such wicks becoming during the combustion soaked with melted tallow, and the top becoming loaded with a deposit of carbon soon bend, and the rays of caloric being directed too much upon the candle, melt the tallow too fast, causing it to waste. The wick therefore requires not less than 8 threads, the strength of which is sufficient to keep it upright till it reach the required length, when it bends and the extremity is gradually burnt off. *

The extremity of the wick of a common tallow candle in burning, continues in the centre of the flame beyond the proper time, and receives such a deposit of carbon as to increase its size greatly, and hence the light becomes diminished at least one-half. This has been obviated in some measure by placing the candle in a position inclining from the perpendicular. But as the angle of inclination necessary to obviate the inconvenience is not less than 30° the suggestion has been seldom adopted. One principal object I had in view in making the experiment now detailed, was to form a tallow candle, so that this advantage might be obtained more readily. As a tallow candle with two wicks gives nearly the same light as a wax candle, it seems better fitted for accomplishing this end than one with three wicks; it was with it therefore that my observations were made. I found that when placed exactly upright, sometimes the wicks either did not bend sufficiently soon, or inclined in opposite directions with regard to each other, and assumed a shape which was unpleasant to the eye. It seemed necessary on that account to give the candle a position somewhat inclined, and I found that an angle of not more than ten degrees was sufficient, the wicks being placed in a plane with each other, and I generally in lighting the candle gave it first a slight bend. With these arrangements I partially succeeded, such a candle placed in this manner, burning without requiring snuffing, and the wicks when consisting of 8 or 10 threads possessing sufficient strength to retain a straight form till they acquire sufficient length, when the extremity bends and is consumed. More

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* The cotton which I used was that sold in the shops for making the best mould tallow candles; 8 threads form a wick about the thickness of that of a common wax candle.

extensive and varied observations than I have been able to make are necessary to ascertain the comparative advantages and disadvantages which such a tallow candle possesses, compared with a common tallow candle, so as to render it fit or unfit for use under peculiar circumstances. I apprehend, however, that though it may answer perfectly well when used in cold and temperate weather, and when there is no wind to affect the direction of the flame; in hot weather or when the atmosphere is not still, it will be found apt to gutter; for, on account of circumstances which further investigations are required to explain satisfactorily, tallow is very liable, particularly in warm weather, to melt in too large quantity at the surface and to run down the sides of the candle.

ART. XXI.—ANALYSIS OF SCIENTIFIC BOOKS AND MEMOIRS.

I.—*Principles of Geology, being an attempt to explain the former changes of the Earth's Surface by reference to causes now in operation.* By CHARLES LYELL, Esq. F. R. S. Foreign Secretary to the Geological Society. In two volumes. Vol. I. London, 1830. Pp. xv. and 511.

UNSETTLED as is the present state of geology, and prudent as may be the line most generally adopted in Britain since the establishment of the Geological Society, of publishing researches upon individual classes of phenomena, rather than forming large and imperfect attempts at complete systems, we are not sorry to see an effort to embody the multifarious facts, which, during a long period, have been accumulating in the annually increasing piles of Transactions and periodical works; more especially when undertaken by a man of the scientific character of Mr Lyell, and in the responsible situation which he holds. For some years, no work affecting to be a complete geological manual (for we rather look upon the volume before us as a judicious compilation, than as a work of much originality,) has issued from the British press, unless we should except one of recent date and of unbounded pretensions, which has been characterized by a distinguished president of the Geological Society of London as a work " in which the worst violations of philosophic rule, by the daring union of things incongruous, have been adopted by the author from others, and at the same time decorated by new fantasies of his own." Some better specimen of British talent was therefore desirable to do away the unfavourable impression which might still attach to the national scientific character, even after the misty light shed by the " New System" in some corners of the speculative world had been finally extinguished by the anathema of one of the first authorities in Europe.

We turn, therefore, with pleasure to a work having the recommendations of great sound sense, industry in compilation, and a pleasing and generally perspicuous style in execution. We are, however, of opinion that it ought not to have appeared without the second volume; which, indeed, we can hardly suppose will be sufficient to contain the remainder of this most extensive subject on the same scale of fulness of illustration with the bulky introductory one before us. Mr Lyell's arrangement also appears to us a faulty one. Geology is a science which ought to be, and in fact *must* eventually be, treated analytically. Mr L. attempts to treat it synthetically, and the greater part of this volume is occupied, after assuming an hypothesis, with collecting data and postulates for constructing his propositions; and almost the whole application to phenomena is left to the latter portion of his work. He ought first to have described the features of the crust of the globe, and from the natural facts they present, deduced the probability of one assumption in preference to all others; then developing the results of this hypothesis, descended again to phenomena, and proved the agreement which he asserts. Now in this first volume, he prevents the reader (unless he be previously acquainted with the subject) from doing any thing but following him; he treats opinions with which he does not agree as unfounded dogmas, and directs the mind almost exclusively to those facts which bear upon his particular views; indeed, how could he do otherwise, unless by stating at the commencement the actual appearances of the crust of the earth, unconnected with any theory whatever? But this would be the analytical method. The other principal fault which we consider runs through the work, and is connected with the former, is an appearance of prepossession in favour of a theory almost unavoidable from the arrangement adopted, which induces him to dwell quite disproportionately upon effects confirmatory of his system, and to pass lightly over its difficulties; which are intended, we presume, to be stated more fully in order to refutation, in the ensuing volume.

The arrangement of Mr Lyell's plan, so far as it is contained in the present portion of his work, appears to be the following:—After stating the objects of the legitimate science of geology, he gives a rapid sketch of its history, and then, in a chapter on the causes which have retarded its progress, brings forward his *assumption*, which the remainder of the work is intended to prove, that causes at present in operation are sufficient to account for the whole phenomena of structure observable in the strata of our globe, provided sufficient time be allowed. Foreseeing that the testimony of organic remains, so strongly indicative of a change of climate since the larger part of the crust of the earth was deposited, would be most apt to occur as a contradiction to his hypothesis, he dips into the chapter of phenomena, and selects this, and this alone, for confutation. He endeavours to prove that the change of climate is referrible to causes at present active. He then assaults the argument which the proofs of the former objection raised up, that the types of organization in the animal and vegetable kingdom have distinctly and undeniably changed; and here again from his mode of arrangement, his argument on this important subject is left incomplete, and the greater part of it deferred to another portion of the work. He throws down at once, with an abruptness little short of the *ex cathedrâ fiat*, the entire theory of the progressive advancement in organization of fossil remains, as we ascend through the strata from the older deposits to the uppermost or latest, and thence infers, that no change in the laws of nature can be argued from a mere change in the species which now inhabit the waters and the land. Finally, he denies any argument derived from the recent appearance of man, which he reconciles to the hypothesis, by calling in the aid of metaphysical reasoning, now rather an exploded instrument in geological inquiry. Mr Lyell next divides the changes at present going forward, as they affect the organic and inorganic world; and the remainder of the volume is occupied by the consideration of the latter, which he divides into aqueous and igneous, the discussion of which forms, we think, by much the happiest part of the work, and, if placed in rather too prominent a view, is a least both instructive and interesting.

After this general outline of the contents of the volume, we shall proceed to notice some parts of it more particularly, and make several extracts. The limits to which we chance to be confined in this Number, will not permit us to do more than commence our remarks, which will be concluded in a second notice in our ensuing Number.

We shall not enter upon the subject of the history of geology, contained in the first chapter of the work: We will only say that Mr Lyell appears, in some instances to have allowed his theoretical opinions to detract too much from the real merits of his opponents, and perhaps is too solicitous to gather from the loose speculations of older geologists, the germs of all that does most honour to the modern progress of the science. It would be strange, if among vast masses of hypothetical errors; were not to be found some guesses at truth; and we have only to consult the ingenious works of Dutens, to see how easy it is to assert plausibly, that Newton, Locke, and Galileo, were only plagiarists from Pythagoras,. Aristotle, and Ptolemy. Mr Lyell supports, on the whole, the Huttonian theory, though he will not admit the introduction of new agents, or even the greater magnitude of existing ones in the formation of the strata, and gives to his predecessors much of the merit usually assigned to Hutton. The progress of geology of late years is next duly noticed, though perhaps the following is hardly an impartial picture of its present state.

"When we compare the result of observations in the last thirty years with those of the three preceding centuries, we cannot but look forward with the most sanguine expectations to the degree of excellence to which geology may be carried, even by the labours of the present generation. Never, perhaps, did any science, with the exception of astronomy, unfold, in an equally brief period, so many novel and unexpected truths, and overturn so many preconceived opinions. The senses had for ages declared the earth to be at rest, until the astronomer taught that it was carried through space with inconceivable rapidity. In like manner was the surface of this planet regarded as having remained unaltered since its creation, until the geologist proved that it had been the theatre of reiterated change, and was still the subject of slow but never ending fluctuations. The discovery of other sytems in the boundless regions of space was the triumph of astronomy—to trace the same system through various transformations—to behold it at successive eras adorned with different hills and valleys, lakes and seas, and peopled with new inhabitants, was the delightful meed of geological research. By the geometer were measured the regions of space, and the relative distances of the heavenly bodies—by the geologist myriads of ages were reckoned, not by arithmetical computation, but by a train of physical events—a succession of phenomena in the animate and inanimate worlds—signs which convey to our minds more definite ideas than figures can do, of the immensity of time." P. 73.

Perhaps the science was never more unsettled than at this moment, but it is in that species of confusion which betokens the resolution to lay the foundations of it on a firmer basis, and so far it bears a promising aspect; yet year after year we see propositions, once considered the most undoubted, confuted by the progress of careful research, and we are sometimes almost tempted to doubt the existence of fixed principles at all. But the time has passed when genuine philosophers are content to gloss over obstinate facts in order to obtain a short cut to ultimate conclusions, and the volumes of the *Geological Transactions* contain a mass of laboriously gleaned facts, which, however contradictory or perplexing they may often appear, will at last, we may hope, settle down into a firm basis for a superstructure to be raised by strict induction. Be it ever remembered, that the anomalies which seemed to threaten the very existence of the Newtonian system of the universe, finally supported it by the most irrefragable of all testimony.

In discussing causes which have retarded the progress of the science, Mr Lyell considers the reluctance to admit a sufficiently long tract of time, for the accomplishment of geological revolutions, the greatest. The following is one of the most plausible points of view in which he placed this fundamental opinion.

"One consequence of undervaluing greatly the quantity of past time is the apparent coincidence which it occasions of events necessarily disconnected, or which are so unusual, that it would be inconsistent with all calculation of chances to suppose them to happen at one and the same time. When the unlooked for association of such rare phenomena is witnessed in the present course of nature, it scarcely ever fails to excite a suspicion of the preternatural in those minds which are not firmly convinced of the uniform agency of secondary causes;—as if the death of some individual in whose fate they are interested, happens to be accompanied by the appearance of a luminous meteor, or a comet, or the shock of an earthquake. It would be only necessary to multiply such coincidences indefinitely, and the mind of every philosopher would be disturbed. Now it would be difficult to exaggerate the number of physical events, many of them most rare and unconnected in their nature, which were imagined by the Woodwardian hypothesis to have happened in the course of a few months; and numerous other examples might be found of popular geological theories, which require us to imagine that a long succession of events happened in a brief and almost momentary period." P. 80

Here our author, seeing the difficulties which must be opposed to his idea of the formation of strata by mere aqueous degradation, brings in the undeniable fact of the uniformity of arrangement in the strata of the whole globe, (of which, however, he has yet given no exposition, without which the reader ought to be supposed ignorant of the fact;) and proceeds to rid himself of this difficulty.

"When it was imagined that sedimentary mixtures, including animal and vegetable remains, and evidently formed in the beds of ancient seas, were of homogeneous nature throughout a whole hemisphere, or even farther, the dogma precluded at once all hope of recognizing the slightest analogy between the ancient and modern causes of decay and reproduction. For we know that existing rivers carry down from different mountain-chains sediment of distinct colours and composition; where the chains are near the sea, coarse sand and gravel is swept in; where they are distant, the finest mud. We know, also, that the matter introduced by springs into lakes and seas is very diversified in mineral composition; in short, contemporaneous strata now in the progress of formation are greatly varied in their composition, and could never afford formations of homogeneous mineral ingredients co-extensive with the greater part of the earth's surface. This theory, however, is as inapplicable to the effects of those operations to which the formation of the earth's crust is due, as to the effects of existing causes. The first investigators of sedimentary rocks had never reflected on the great areas occupied by modern deltas of large rivers; still less on the much greater areas over which marine currents, preying alike on river-deltas, and continuous lines of sea-coast, might be diffusing homogeneous mixtures. They were ignorant of the vast spaces over which calcareous and other mineral springs abound upon the land and in the sea, especially in and near volcanic regions, and of the quantity of matter discharged by them. When, therefore, they ascertained the extent of the geographical distribution of certain groups of ancient strata—when they traced them continuously from one extremity of Europe to the other, and found them flanking, throughout their entire range, great mountain-chains, they were astonished at so unexpected a discovery; and, considering themselves at liberty to disregard all modern analogy, they indulged in the sweeping generalization, that the law of continuity prevailed throughout strata of contemporaneous origin over the whole planet. The difficulty of dissipating this delusion was extreme, because some rocks, formed under similar circumstances at different epochs, present the same external characters, and often the same internal composition; and all these were assumed to be contemporaneous until the contrary could be shown, which, in the absence of evidence derived from direct superposition, and in the scarcity of organic remains, was often impossible."—Pp. 90, 91.

Here Mr Lyell seems fairly to entrench himself in his *deltas* and defy attack, and while he enlarges on the great *absolute* magnitude of these modern deposits, upon which, as we shall afterwards see, he collects many interesting particulars, he forgets, and would have the reader forget, their *relative* insignificance when compared to the continental masses which he conceives to have had a similar formation;—so insignificant indeed that none of them are more than specks in the map of the world, and yet these are the produce of the continued action of 6000 years. We know not whether it is boldest to call into the aid of a theory, powers of mechanical action greatly surpassing those at present witnessed in the economy of nature, or periods of time almost transcending our powers of conception; our author seems to think the latter no assumption at all, and the former an unwarrantable one; but we do not see this difference, and we are much disposed to agree with those distinguished writers who consider the former hypothesis, when applied to conformations, so universal as the superposition of rocks, at least as plausible as the latter.

In the sixth chapter Mr Lyell proceeds to prove that a change of climate has taken place in high latitudes, by a reduction of temperature, since the older strata were deposited,—a position so generally, (though not universally,) admitted, that we shall not dwell upon it. In the succeeding chapters he attempts to show that such a change might be accounted for on simply physical principles. Considerable details are entered into to show that the existence of land, and especially high land, in northern latitudes, tends to the refrigeration of the zones betwixt them and the equator,—a fact sufficiently illustrated by the great decrement of temperature in the vast tracts of North America and Siberia, below that of the corresponding latitudes in Europe and other countries, where an equilibrium is assisted by the maritime situation; therefore it is conceived that the amount of land in high latitudes is the proper index to the climate. "It next remains for us," says Mr Lyell, "to inquire whether the alterations which the geologist can prove to have actually taken place at former periods in the northern hemisphere, coincide in their nature, and the time of their occurrence, with such revolutions in climate as would naturally have followed, according to the meteorological principles already explained." It is impossible for us to follow Mr Lyell through this rather intricate chapter, or to point out what we consider the weak points of his argument, but we will give some of his deductions in his own words. "We may observe, that although geologists have neglected to point out the relation of changes in the configuration of the earth's surface with fluctuations in general temperature, they do not dispute the fact, that the sea covered the regions where a great part of the land in Europe is now placed, until after the period when the newer groups of secondary rocks were formed. There is, therefore, confessedly a marked coincidence in point of time between the greatest alteration in climate and the principal revolution in the physical geography of the northern hemisphere. It is very probable that the abruptness of the transition from the organic remains of the secondary to those of the tertiary epoch, may not be wholly ascribable to the present deficiency of our information. We shall doubtless hereafter discover many intermediate gradations, (and one of these may be recognized in the calcareous beds of Maestricht,) by which a passage was effected from one state of things to another; but it is not impossible that the interval between the chalk and tertiary formations constituted an era in the earth's history, when the passage from one class of organic beings to another was, comparatively speaking, rapid. For if the doctrines explained by us in regard to vicissitudes of temperature are sound, it will follow that changes of equal magnitude in the geographical features of the globe, may at different periods produce very unequal effects on climate, and, so far as the existence of certain animals and plants depends on climate, the duration of species may often be shortened or protracted, according to the rate at which the change in temperature proceeded."—P. 139. It is, however, impossible not to remark, that, notwithstanding the manifold elevations which appear to have taken place between the deposition of the mountain limestone and the Maestricht deposit above chalk, the corallines of the one, and the fossil turtles of the other, point equally to a tropical temperature. The change in the conformation of organic remains, as indicative of temperature, seems abrupt, and confined to the newest formations.* How far Mr Lyell's deductions on this curious subject may be adopted by geologists we are doubtful, but they deserve the credit of being striking and ingenious speculations.

In our next Number we shall meet Mr Lyell on less debateable ground, and hope to make more extended and popular extracts from his interesting volume.

II.—*The Elements of Experimental Chemistry.* By WILLIAM HENRY, M. D., F. R. S. &c. &c. The Eleventh Edition, comprehending all the recent Discoveries, &c. In two Volumes. London, 1829. With plates and Engravings on Wood.

FROM this new edition of Dr Henry's system of chemistry, we extract the following recommendation, which is addressed to the notice of learned Societies. "The great laws of combination in definite and in multiple proportions, on which the ATOMIC THEORY mainly rests, have, more especially, derived increased support from the accumulated mass of evidence. In too many instances, it must be acknowledged, we have not, even yet, attained all the precision that is desirable, as to the true proportions in which bodies combine. Nor can we arrive at this degree of certainty, until the relative weights of some of the elementary gases have been determined, with the aid of the most refined instruments, and with the most elaborate and scrupulous correctness. It were to be wished, indeed, that this should be attempted under the auspices of some one of those learned societies, which have been instituted for the promotion of science; and that the investigation should be confided to a commission of its members, whose skill, experience, and fidelity, would be a pledge for the accuracy of the results. The precise admeasurement of an arc of the meridian was not more important to astronomical truth, than the exact determination of the specific gravities of the elementary gases is to chemical philosophy."

With the importance of the foregoing observation we concur; and should be proud if the chief philosophical institution of our Scottish metropolis would take the lead in putting into execution so desirable an object.

In the preface, the author has alluded to the deep loss which the scientific world has sustained by the death of Sir Humphry Davy and Dr Wollaston, in a joint eulogium upon these two distinguished philosophers, which is characterized no less by its just discrimination of their respective excellencies, than by its forcible eloquence: "It is impossible" says Dr Henry

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* Of this Mr Lyell has himself furnished evidence in reference to some of the Subapennine formations in his Sixth Chapter.

" to direct our views to the future improvement of this wide field of science, without deeply lamenting the privation, which we have lately sustained, of two of its most successful cultivators, Sir Humphry Davy and Dr Wollaston,—at a period of life, too, when it seemed reasonable to have expected, from each of them, a much longer continuance of his invaluable labours. To those high gifts of nature, which are the characteristics of genius, and which constitute its very essence, both those eminent men united an unwearied industry and zeal, and research, and habits of accurate reasoning, without which even the energies of genius are inadequate to the achievement of great scientific designs. With these excellencies, common to both, they were nevertheless distinguishable by marked intellectual peculiarities. Bold, ardent, and enthusiastic, Davy soared to greater heights; he commanded a wider horizon; and his keen vision penetrated to its utmost boundaries. His imagination, in the highest degree fertile and inventive, took a rapid and extensive range in pursuit of conjectural analogies, which he submitted to close and patient comparison with known facts, and tried by an appeal to ingenious and conclusive experiments. He was imbued with the spirit, and was a master in the practice, of the inductive logic; and he has left us some of the noblest examples of the efficacy of that great instrument of human reason in the discovery of truth. He applied it, not only to connect classes of facts of more limited extent and importance, but to develope great and comprehensive laws, which embrace phenomena, that are almost universal to the natural world. In explaining those laws, he cast upon them the illumination of his own clear and vivid conceptions;—he felt an intense admiration of the beauty, order, and harmony, which are conspicuous in the perfect CHEMISTRY OF NATURE;—and he expressed those feelings with a force of eloquence, which could issue only from a mind of the highest powers, and of the finest sensibilities. With much less enthusiasm from temperament, Dr Wollaston was endowed with bodily senses of extraordinary acuteness and accuracy, and with great general vigour of understanding. Trained in the discipline of the exact sciences, he had acquired a powerful command over his attention, and had habituated himself to the most rigid correctness, both of thought and of language. He was sufficiently provided with the resources of the mathematics, to be enabled to pursue, with success, profound enquiries in mechanical and optical philosophy, the results of which enabled him to unfold the causes of phenomena, not before understood, and to enrich the arts, connected with these sciences, by the invention of ingenious and valuable instruments. In CHEMISTRY, he was distinguished by the extreme nicety and delicacy of his observations; by the quickness and precision, with which he marked resemblances and discriminated differences; the sagacity with which he devised experiments, and anticipated their results; and the skill, with which he executed the analysis of fragments of new substances, often so minute as to be scarcely perceptible by ordinary eyes. He was remarkable, too, for the caution, with which he advanced from facts to general conclusions; a caution which, if it sometimes prevented him from reaching at once to the most sublime truths, yet rendered every step of his ascent a secure station, from which it was easy to rise to higher and more enlarged inductions.

Thus these illustrious men, though differing essentially in their natural powers and acquired habits, and moving, independently of each other, in different paths, contributed to accomplish the same great ends—the evolving new elements; the combining matter into new forms; the increase of human happiness by the improvement of the arts of civilized life; and the establishment of general laws, that will serve to guide other philosophers onwards, through vast and unexplored regions of scientific discovery."

The foregoing interesting extracts from the new edition of Dr Henry's chemistry are sufficient. To enter into an analysis of such a well known standard work as this, proceeding from the pen of one who ranks among the most eminent chemical philosophers of the day, would indeed be a superfluous task. We remember many years ago, in a very different chemical era to the present, when the first edition of this work appeared under the unpretending form of a duodecimo volume, intended as a manual for the experimental student. From this time, Dr Henry has been an unremitting labourer in the field of science, and as his work in its successive editions has kept a regular pace with the advances of chemical knowledge, to which he has himself been so distinguished a contributor, the eleventh edition now appears before the public in a very enlarged and ample form, containing a store of information, the selection and arrangement of which cannot be too highly rated. In short, Dr Henry is to be esteemed as an author, who has always been an industrious collector of facts, and an accurate reasoner; avoiding premature speculations, and strenuous for the rigid canons of inductive philosophy. For this reason, his volumes may be recommended as among the most useful and the safest which can be entrusted to the hands of the student.

ART. XXII.—PROCEEDINGS OF SOCIETIES.

1. *Proceedings of the Royal Society of Edinburgh.*

March 15, 1830.—The following communications were read :—

1. Dr Knox concluded the Second Part of his paper on Hermaphroditical Appearances.

2. Dr Christison read a paper entitled, " An Experimental Inquiry into certain doubtful points in the Physiology of the Blood and Respiration. Part I. On the Mutual Action of the Blood and Atmospheric Air."

April 5. The following Gentlemen were duly elected Ordinary Members of the Society :—

The Hon. Mounstuart Elphinstone.

James Syme, Esq.

Thomas Brown Esq. of Langfine.

Dr Gregory read the translation of a paper, entitled, " On the Mechanism by which the Modulation of the Human Voice is affected in Singing." By Francisco Bennati. M. D. of the Universities of Padua and Pavia.

April. 19.—The following communications were read :—

1. Account of observations made in Scotland on the Distribution of the Magnetic Intensity, by Mr Dunlop.

2. A notice regarding a method of producing Continuous Motion in the circumference of a Ellipse, with its application to the construction of an Elliptograph. By Professor WALLACE. An Elliptograph and a Cometarium were exhibited.

2. *Proceedings of the Society for the Encouragement of the Useful Arts in Scotland.*

The following communications have been read and exhibited to the Society since 17th Feb. 1830:—

March 3, 1830.—1. A Model and Description of a Cart to be propelled by levers and cranks, acted on by the weight and force of one man, *without* a horse. By WILLIAM ALLAN, Morningside.

2. Description of a Slow Motion for the beam compass. By Mr EDWARD SANG, teacher of mathematics, Edinburgh, M. S. A.

March 17.—1. An account of the latest improvements in the turning-lathe, including the slide-rest, and apparatus for drill-turning; as also an account of the planing-engine and apparatus, with engravings. Communicated by John Robison, Esq. Sec. R. S. E. and M. S. A.

2 Drawing and description (as amended) of a simple, cheap, and accurate rain-gage. By Matthew Adam, A. M. Rector of the Academy of Inverness and Assoc. S. Arts. See this Vol. p. 53.

3. Description of a Pendulum Chronometer, in which the arbors of the wheels move on friction rollers, and the pinion leaves are made so as to revolve by the impulse of the wheel teeth, which are of a peculiar form, Made by DAVID WHITELAW, watch and clock-maker, 16, Prince's Street. Edinburgh, for the late Andrew Waddell, Esq. Hermitage Hill, Leith. Communicated by the late Mr Waddell.

March 31.—1. Observations on the application of heated air to the warming of dwelling-houses and of churches, hospitals, and other public buildings; with remarks on various kinds of stoves used for this purpose. By Mr Robert Ritchie, ironmonger to his Majesty, High Street, Edinburgh. Models of the stoves, &c. were exhibited.

2. Description of an improved levelling rod. By Mr JAMES FLINT, civil engineer, Terrace, Edinburgh. The rod was exhibited.

3. Additional observations on safety windows for upper stories of houses. By Thomas Johnston, ink-manufacturer, Glasgow.

April. 14.—1. Remarks on the Eidograph, Pentagraph, &c. were read. By Mr Professor Wallace, F. R. S. E. and M. S. A. The instruments were exhibited.

2. Notices of various plans of applying heat, either by common fire places, or by steam apparatus, were communicated by Mr Robert Ritchie, ironmonger to his Majesty, High Street, Edinburgh. Models of various stoves, &c. and of a drying house were exhibited.

3. Notice of an apparatus for facilitating the making of infusions by hot water, and particularly from coffee. By John Robison, Esq. Sec. R. S. E. and M. S. A.

George Mackillop, Esq. Ainslie Place, and Mr Adam Wilson, smith, Mint, were admitted Ordinary Members.

April. 28.—1. Notice respecting Mr Cuthbert's Elliptic Metals for Reflecting Microscopes. Communicated by Dr Brewster, F. R. S. E, and M. S. A. See this *Journal*, No. iv. p. 321.

2. Investigation of the Spherical Abberration of a Diamond Lens. By Mr Andrew Pritchard, London, Hon. M. S. A. for Scotland. Communicated by Dr Brewster, F. R. S. E. and M. S. A. See No. iv. p. 317.

3. Description of the improvements on the common mortice lock. By Mr JAMES WILLIAMSON, Melrose, Assoc. Soc. Arts. The Lock was exhibited.

4. Description of a method of destroying Vermin on Fruit Trees, Bugs, &c. by means of Steam. By James Grieve copper-smith and brazier, 20, Greenside Place, Edinburgh. The apparatus was exhibited.

5. Notice regarding the improved steam and indicator and oil test. By John M'Naught, engineer, Glasgow.

6. Memorial on the construction of Chimnies, so as to prevent Smoke. By Alexander Mollison, Englinton Street, Glasgow.

7. Notice of a Swiss Lock, of neat, simple, and efficient construction, made by Mr CORMACK, smith, Chalmers's Close, for and presented to the Society by Sir ALEXANDER MUIR MACKENZIE of Delvine, Bart. The lock was exhibited.

Sir ALEXANDER MUIR MACKENZIE of Delvine, Bart. was addmitted an Ordinary Member.

The following donations were laid on the table, viz.—

1. Microscopic illustrations. By C. R. Goring, M. D, and ANDREW PRITCHARD, Hon. M. Soc. Arts for Scotland; with Coloured Engravings. Presented to the Society of Arts by Mr Pritchard.

2. Plans for the floating off of stranded vessels, and for raising those that have foundered; with an improved method of carrying vessels over banks in shallow water. By JOHN MILNE, teacher of architectural and mechanical drawing, Edinburgh, and Curator Soc. of Arts for Scotland. Presented by the Author.

3 *Proceedings of the Cambridge Philosophical Society.*

March 22, 1830.—The Rev. Professor Sedgwick, Vice-President in the chair. A notice by Mr Miller of St John's was read, on the measurements of certain crystals found in the slags of furnaces at Merthyr Tydvil and at Birmingham. The form and angles of these crystals were ascertained to be the same as those of olivine: and agree therefore with those of crystals found by Prof. Mitscherlich in the forges of Sweden and Germany.

Mr Coddington gave a further explanation of the construction of his newly-invented microscope, and of the superiority of its performance, which was shown by a comparison with a large microscope on the usual construction made by Dollond. See this volume, p. 155.

A paper by H. K. Cankrien, Esq. of Trin. Coll. was also read, "on the calculus of variations."

After the meeting, Mr Willis gave an account, illustrated by models and drawings, of the various organs which compose the apparatus of deglutition and vocalisation. He explained in particular the various muscles of the palate, pharynx, and tongue; and the forms which these parts assume in the course of their various functions were exhibited as drawn from actual measurement. Mr Willis further explained the distinction of the parts which are employed in producing the musical note of the voice, and those which determine its vowel quality.

April 25.—The Rev. Professor Sedgwick in the chair. Among other presents laid before the society, was a collection of the eggs of British birds, the gift of Mr Yarrell.

The Rev. L. Jenyns read a communication on the subject of the late severe winter.

The Rev. H. Coddington read a memoir on the subject of his improved microscope, which was again exhibited and tried on several of the usual test objects, (striated scales or feathers of different butterflies and moths.)

Professor Whewell made some observations on the proof of the first law of motion.

After the meeting, Professor Whewell gave an account of the arguments brought forward by the German writers who reject the Newtonian theory of optics, and of the doctrines on this subject propounded by the celebrated Goëthe.

It has been very common of late years among German writers to speak of the Newtonian doctrine of the separation of white light into colours by refraction, as of a system which is palpably and even ridiculously false; retained only through the influence of the most perverse blindness and prejudice; and rejected by all persons of philosophic views. A different theory has been maintained by the celebrated Goethe in his "*Farbenlehre*," and has met with very considerable success in his own country. The confident and triumphant manner in which the advocates of the new system are in the habit of speaking, is calculated to excite some curiosity as to their arguments in the minds of those who have been accustomed to consider the Newtonian principles as established upon an unimpeachable series of inferences from experiment.

Goethe has criticised, page by page, a great part of Newton's *Optics*, and brings against the author charges, which are perpetually repeated by other writers, of mis-stating and misinterpreting his experiments, of confounding simple and complex phenomena, and even of bad faith. The main argument, however, is, that the asserted separation of white light by the prism never takes place except at the boundary of a bright surface; and that, therefore, darkness as well as light is requisite for the production of the coloured image. The colours, it is maintained, are always a fringe to the image, and prove no difference of refrangibility in the rays of light.

Goethe's theory of the cause of the coloured borders produced by a prism may be stated as follows: 1° *Light* seen through a *dim transparent medium* appears yellow, orange, and red, according to the thickness and transparency of the medium;—*darkness* seen through a similar medium, exhibits in a similar succession, blue and violet tints. 2° When refraction takes place, the principal image is accompanied by an *accessory image,* ("nebenbild,") which is near and similar to the principal image, but is carried farther than that is in the direction of the refraction. 3° This accessory image being but, as it were, half an image, may be looked upon as a dim transparent medium, such as is mentioned in 1°. 4° Hence, at that end of the bright image which is foremost in the direction of the refraction, the accessory image will encroach upon the dark surrounding space, and will produce blue and violet, as in 1°, at the other extremity of the image, the accessory image will be drawn over part of the bright image, and will produce red and yellow. Hence we have a border blue and so forth at one end, and a yellow rim at the other, of a bright object when refracted. 5° The green in the middle of the spectrum is produced by the mixture of the yellow of one end, with the blue of the other, when the image is of such a size as to allow them to coincide.

It was not considered necessary to compare the two theories, or to make any remarks on the above arguments against the Newtonian reasonings.

May 10.—Dr F. Thackeray, the Treasurer, in the chair. Various additions to the Society's collection of British birds, presented by the Rev. L. Jenyns, and some specimens of insects, presented by Mr Dale, were laid before the meeting.

A paper by T. W. Chevalier, Esq. on the Anatomy and Physiology of the Ear, was read.

After the meeting, Professor Cumming exhibited and gave an account of some philosophical instruments which have recently been invented or improved. He described the contrivance proposed by Professor Leslie for measuring the specific gravity of powders; and pointed out the resemblance between the instrument on this account termed a *koniometer,* and the *stereometer* invented by M. Say in 1797. Prof. Cumming explained also a method of applying a similar process in a more convenient and compendious manner by means of the air-pump. An account was likewise given of the apparatus of Mr Meikle, for comparing the specific gravities of two fluids; and an improvement in its construction pointed out.

Professor Cumming exhibited to the members an instrument, the object of which is to measure the total effect of the whole sunshine which occurs in the course of a given day or any other time.

ART. XXIII.—SCIENTIFIC INTELLIGENCE.

I. NATURAL PHILOSOPHY.

MAGNETISM.

1. *On the production of Magnetism by Friction.* By M. HALDAT.—Friction has been long known to be capable of producing magnetism, but it was not supposed to be efficacious, unless upon iron either magnetised or in a neutral state. M. Haldat of Nancy has, however, found that all hard bodies may, by means of friction, assist in the decomposition of the mag-

netic fluid, if their action is promoted by the combined action of magnets, which, by themselves, are incapable of producing it. To prove this, take a piece of soft iron wire, a decimeter long, (about 4 inches,) and a millimeter (1-25th of an inch) in diameter. If this wire is placed horizontally between two bar magnets, with their opposite poles facing one another, and at such a distance that it cannot be magnetised, it will receive distinct magnetism by friction with all hard bodies, such as copper, brass, zinc, glass, hard woods, &c.—*Ann. de Chim.* tom. xlii. p. 41.

II. CHEMISTRY.

2. *Mineral Kermes.*—The composition of mineral kermes, as determined by Berzelius and Rose, in accordance with Philips, has lately been called in question by the French chemists. According to the former chemists, it is exactly the same substance as the common native sulphuret of antimony. Robiquet, Buchner, and Henry Junior, who found it to contain oxide of antimony, have been joined by Gay-Lussac, who says, (*An. de Chim. et de Phys.* xlii. p. 87.) that, when heated with hydrogen gas, it gives off water, and that it is in fact a compound of 1 atom oxide of antimony +2 sulphuret of antimony. Rose has therefore repeated his experiments (Poggendorff's *Annalen*, xvii. 324.) with his former results. He prepared his kermes by boiling carbonate of soda on the common sulphuret of antimony, filtering, setting aside to cool, filtering again in half an hour to collect the precipitate, drying it well on bibulous paper, and afterwards by a gentle heat till it ceased to lose weight. He found the kermes thus prepared to give no water in a current of hydrogen gas, but to leave 72.71 per cent. of metallic antimony. His former analysis gave 72.32 per cent., and Berzelius found in the common sulphuret 72.77 per cent. The residual liquid, according to Rose, after some hours, becomes troubled, and deposits a white sediment, being oxide of antimony combined with soda. If the kermes be not filtered soon after its deposition, it will thus be contaminated both with oxide of antimony and with alkali,—and this is probably the source of the oxide found by the French chemists. Since no carbonic acid is evolved during the preparation of the kermes by this process, all that takes place is a mere solution of the sulphuret of antimony in the carbonated alkalies.

3. *Phosphuret of Sulphur.*—When the proto-chloride of phosphorus is exposed in like manner to the action of sulphuretted hydrogen—heat is evolved, and there is formed a solid yellow substance without any apparent crystalline form, and adhering strongly to the glass. This is a phosphuret of sulphur. At common temperatures it decomposes water, and at length disappears in it, forming sulphuretted hydrogen and phosphoric acid. Its atomic constitution is probably [*]

2 atoms phosphorus + 3 atoms sulphur.

Our readers will remember that Faraday and Mitscherlich have described another compound of these elements, consisting of

2 atoms phosphorus + 1 atom sulphur.

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[*] *An. de Chim. et de Phys.* xlii. p. 25.

4. *Chlorides, Iodides, and Bromides of Sodium.*—The bromides and iodides of sodium crystallize from their solutions at common temperatures with water of crystallization—common salt first at a temperature several degrees (14° to 17° Fahr.) below the freezing point. Anhydrous crystals of the bromide are obtained from a solution at a temperature of about 86° Fahr. and of the iodide at a temperature from 104° to 122° Fahr. The anhydrous crystals of these three salts of sodium are perfectly alike, being cubes in which, though seldom, as is known to be the case with common salt secondary faces, are found.

Of the three hydrous salts the chloride forms the finest crystals, but the determination of their angles is very difficult, since at a temperature above 14° Fahr. they deliquesce, and above 32° they lose their water, which, when it has previously become liquid, dissolves a portion of the salt. The crystals of the hydrous bromide is not changed at common temperatures, and can be measured with great ease,—the hydrous iodide, on the contrary, deliquesces, and is very difficult to measure, nevertheless I have succeeded in measuring all of them by the reflecting goniometer. They are all isomorphous, and the angles differ from each other by no appreciable quantity. The number of faces and the modes of formation of the crystals are also precisely the same. They are commonly obtained in the form of flat tables. The primitive form is an oblique rhombic prism.

M on M′ = 118°.32′
M on P = 109 .48

As the crystals contain water from the mother liquor between these plates, it is not easy to determine with precision the amount of water in combination. I have determined it by finding the loss sustained on heating the salt below the melting point. By fusion, a portion is lost, as all the three combinations begin to volatilize at a red heat.

The hydrous chloride consists of		
	Chloride of sodium,	61.98 = 1 atom
	Water,	38.02 = 4
The hydrous bromide of		
	Bromide of sodium,	73.6 = 1 atom
	Water,	26.37 = 4 atoms
The hydrous iodide of		
	Iodide of sodium,	79.77 = 1 atom
	Water,	20.23 = 4 atoms

The hydrous common salt has been already described by Fuchs in his interesting paper on the solubility of common salt in water, but he was unable to determine all the angles.—*Mitscherlich,* Poggend. *An. der Phys. und Chim.* xvii. 385.

5. *Effects of heat upon copper ores.*—In an able paper on the mixed ores of copper, lead, and iron, by Bredberg, a very striking fact is mentioned in regard to the effects of heat upon the sulphuret of copper which they contain. A mass of the ore apparently homogeneous, and containing through

its whole substance from four to five per cent. of copper by roasting, has its structure changed into a succession of layers. In one case cited there were two layers and a central ball. The outer layer had an earthy fracture, and contained only three per cent. of copper. The second layer had the metallic lustre of copper pyrites, and contained twenty per cent. of copper—the internal ball, which contained fourteen per cent. combined with an excess of sulphur. Part of the iron was oxidized, but the proportions of the other constituents in the other parts of the mineral were unchanged. If the roasting be longer continued, a greater portion of the mass will be decomposed, but in the interior will be found a smaller bronze-coloured ball, containing fifty-four per cent. of copper.—*Kongl. Vetenskabs handling*, 1828.

6. *Preparation of Phosphorus.*—Wöhler recommends, as likely to give phosphorus at a very cheap rate, to distil by a strong heat ivory black with half its weight of fine sand and charcoal powder. A silicate of lime is formed, and the carbonic oxide and phosphorus come over.—Pog. *An. de Phys.* xvii. 178.

7. *Hydriodic Ether.*—Serullas gives the following improved process for preparing this substance. Into a tubulated retort are introduced 40 gram. iodine and 100 alcohol of 38° *B.* agitate and add 2.5 grains of phosphorus in small pieces. Distil nearly to dryness; add 25 or 30 grains alcohol, and distil again till nearly dry. Water throws down the ether from the solution. After washing, it is to be distilled from chloride of calcium.—*An. de Chim.* xlii. p. 119.

8. *Seleniuret of Palladium.*—The seleniuret of lead of Tilkerode contains silver and native gold mechanically mixed in dendritic plates, and fine crystalline grains. Assessor Bennecke, the director of a work established on the spot for extracting the selenium and the precious metals from this ore, has obtained from his auriferous solution a considerable quantity of palladium. This has led Zincken * to examine the ore more attentively, to find in what state this last metal occurs. The ore was boiled in nitric acid, which dissolved the seleniuret of lead, and separated many light silver white scales containing palladium. An examination of other specimens of the small scales and dendritic formations of native gold showed him many small scales having the form of six-sided tables, and resembling much the osmuriet of iridium, with here and there little groupes of crystal of a white colour like platina, and a perfectly metallic lustre. It is brittle, has a scaly shining fracture, and cleaves perpendicular to the axis of a six-sided prism. By heat it swells and gives off a peculiar odour, like that emitted by seleniuret of mercury, and forms on the tube a red ring of selenium. By stronger heat a white vapour is given off, and the glass is corroded. With borax it gives a transparent glass, and melts into a brittle bead. This substance is a compound of the seleniuret of

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* Poggendorff, xvi. 495.

lead, silver, and palladium. A closer examination with a compound microscope shows the scales of gold to be covered with minute white crystals of seleniuret of Palladium, invisible to the naked eye. The common seleniurets of silver and lead contain no trace of palladium. Zincken suggests, that the native palladium of Sowerby may contain other ingredients; but the experiments of Wollaston appear sufficiently decisive that it contains no selenium.

9. *Huraulite and Hetepozite.*—Du Frenoy has analyzed these two minerals found formerly by Alluau in the Limoges.

The Huraulite consists of

		Calculation.	Oxygen.	
Phosphoric acid, -	38.00	36.52	8	
Protoxide of iron, -	11.10	11.23	1	4
———— manganese,	32.85	34.95	3	
Water, - -	18.	17.26	6	
	99.95	99.96		

The calculation is after the formula:

$$3\ \dot{M}n^5\ \overset{\cdot\cdot}{\overset{\cdot\cdot\cdot}{P}}_2 + \dot{F}_5\ \overset{\cdot\cdot}{\overset{\cdot\cdot\cdot}{P}}{}^2 + 30\ aq$$

This mineral is in minute crystals of the size of a pin head. The primitive form is an oblique rhombic prism of 117°.30′, and 62° 30′; but it occurs in the form of rhomboidal prisms, with or without the acute angles replaced. It shows no cleavages; has a glassy fracture, and reddish-yellow colour; is transparent; scratches calcspar, but is scratched by steel. Its specific gravity = 2.27. It fuses with great difficulty before the blowpipe, giving a black bead with metallic lustre. In a tube it gives off water. It occurs in the granite about Limoges. The only pieces yet found were picked up by Mons Alluau, near Strassenban.

The Hetepozite is composed of

		Calculation.	Oxygen.
Phosphoric acid,	41.77	42.6	6
Protoxide of iron,	34.89	35.02	2
Protoxide of manganese,	17.57	18.10	1
Water, - -	4.40	4.49	1
Silica, - -	0.22		
	98.35	100.13	

The calculation is according to the formula:

$$2\ \dot{F}^5\ \overset{\cdot\cdot}{\overset{\cdot\cdot\cdot}{P}}_2 + \dot{M}n^5\ \overset{\cdot\cdot}{\overset{\cdot\cdot\cdot}{P}}{}^2 + 5\ aq$$

This mineral is found only in scaly masses, cleaving, however, in three directions, giving an oblique rhombic prism, having an angle of about 100° or 101°. The lustre is shining and fatty, like that of the apatites. The colour greenish-gray or bluish. Weather-worn surfaces have a beautiful violet colour and a semi-metallic lustre. These weather-worn pieces cleave with greater ease, and may be measured with the common goniometer.

The undecomposed scratches glass with ease, but not quartz. In its decomposed state it is softer, and is scratched by steel. The specific gravity of the former is 3.524, of the latter 3.39. It dissolves in acid with a slight residue of silica. Before the blowpipe it melts into a dark-brown enamel. —*Ann. de Chim. et de Phys.* xli. p. 337.

10. *Black Blende of Marmato.*—An analysis of this mineral by Boussingalt, shows it to be a compound of sulphuret of zinc with sulphuret of iron, expressed by the formula .

$$S^2 + 3\,Zn\,S^2$$

Pog. *Ann. der Phys.* xv.

11. *Sulphuret of Silicium.*—Sefström has found that silica heated in his blast furnace in a charcoal crucible can be reduced by sulphuretted hydrogen to a sulphuret of silicium. It is easily volatilized, and in burning gives off the peculiar sublimate of silica, which has been repeatedly met with. This discovery will clear up many phenomena hitherto inexplicable. (Poggen. *An.* xvii. 379.)

12. *New compound of Chlorine, Phosphorus, and Sulphur.*—A new compound of these elements has been formed by Serullas. There are two chlorides of phosphorus, consisting of

	Per-chloride.	Proto-chloride.
Phosphorus	1 atom.	1 atom.
Chlorine	5 atoms.	3 atoms.

When the per-chloride is introduced into an atmosphere of dry sulphuretted hydrogen, it becomes heated, and changes in a short time into a colourless transparent liquid; while muriatic acid vapour takes the place of the sulphuretted hydrogen. When purified by distillation in a small retort, this compound has the appearance of the purest water. It is heavier than water; has a peculiar pungent aromatic smell, mixed with that of sulphuretted hydrogen; fumes slighty in the air, and boils at 125° centigrade. The odour of sulphuretted hydrogen is owing to the action of atmospheric moisture, for when decomposed by oxide of copper, it gives no trace of hydrogen gas. Its composition by the analysis of Serullas is

3 atoms chlorine.
1 atom phosphorus.
1 atom sulphur.

13. *Atomic weight of Iodine and Bromine.*—M. Berzelius has determined that the atomic weight of iodine is 789.145, and the density of its vapour 8.7011. The atomic weight of bromine seems to be about 489.15, and the density of its vapour 5.3983.—*Ann. de Chim.* vol. xl. p. 430.

III. NATURAL HISTORY.

ZOOLOGY.

14. *Queries respecting the Natural History of the Salmon, Sea-trout, Bull-Trout, Herling, &c.*—The value of the Salmon Fisheries in Great Britain has decreased so much of late years, and particularly in the North of England, and South of Scotland, that a remedy for it, independent of its interest as a difficult and unsolved question in Natural History, will become of no little importance to proprietors. The following queries are proposed, with the view and with the hope of gaining some information upon the natural history and economy of this valuable species. It is only by arriving at a correct knowledge of its various habits, and those of the species allied to it, which frequent our rivers in almost equal numbers, that we can hope to devise or accomplish any means of increasing the production, or of decreasing the certainly too extensive destruction of it in its different states.

The queries relate only to its natural history, and answers are earnestly requested, stating facts relative to the opinions given, with the suggestion of additional queries, or any thing that will tend to illustrate the history of the species.

Address the answers to Sir W. Jardine, Jardine-Hall, by Lockerbie, Dumfries-shire.

Salmon.

1. At what age do salmon commence spawning? and how often is it supposed that they have migrated to and from the sea, previous to their first parting with the spawn?

2. Do the males and females attain maturity at the same period or age? and do all of one age spawn nearly at the same season?

3. At what time do the young, or Fry, first leave the rivers?

4. When do the young, or Fry, first return to the rivers?

5. What is the size, weight, and appearance of the Fry, on their first return from the sea, and under what denomination do they then go?

6. Are they so far arrived at maturity as to spawn, and be productive on their first return from the sea, or previous to a second migration?

7. Are any *fish* known to shed their spawn abortively, before they arrive at their full growth or maturity? or is the spawn observable in young fish, retained, until the parents attain the ordinary growth and size of the species when it is known to be productive?

Grilse.

8. Are Grilses immature Salmon, and if they are, what is their age?

9. What is the distinctive character between a *large Grilse* and a *small Salmon*?

10. At what season do Grilses first appear in the rivers? What is their weight? and are they supposed to be the Fry of the same year, on their first return from the sea?

11. Have the Fry been marked, and afterwards taken as Grilses in the course of the same year, and have Grilses been marked, and afterwards taken as full grown Salmon?

12. Is it supposed that any sexual intercourse takes place between the Salmon and other species of the genus, thereby producing a mongrel or mixed breed of fish?

Whitling and Sea Trout.

13. Does the *Whitling* of the Tweed ever become a Salmon—if not, to what size and weight does it attain?

14. Is the *Whitling* of the Tweed known by any other name in its various stages of growth? Does it spawn, and at what season? What are its migrations?

15. Is the *Sea-Trout* of some other rivers the same with the *Whitling* of the Tweed? Is it found in all rivers containing Salmon? Does it spawn? Is the young, or Fry known—and what are its migrations?

*Herling.**

16. Is the *Herling* or *Hirling* of the Annan and Nith, and the *Whiting* of the Esk in Cumberland, the same with the *Finnock* of the west coast of Scotland, and the *Sewin* of the Welsh rivers?

17. Is the *Herling* found in the rivers on the eastern coast of Scotland, or in any of the rivers in England or Ireland, and under what name or names is it there known?

18. Does the *Herling* spawn, and at what season? and is it known in any intermediate state between the Fry and *Herling*? Is the Fry known, and what are its migrations?

Bull Trout.

19. Is the *Bull Trout* of the Tweed the same with the Salmon Trout of the Tyne and Tees, &c.? and is it known by any other name during its growth from the Fry to maturity?

20. Is the *Parr* met with in all rivers containing Salmon? where and when does it spawn? Is it the same with the *Brandling* of the North of England, and the *Skirling* of Wales? Is it supposed to be a perfect fish, or the Fry of some species of Salmon?

21. What is the Grey (*Salmo Eriox*) of Dr Fleming? What are its states from the young to the adult? What are its migrations?

22. Are there any species of migratory Salmon, distinct from those above mentioned, known in the rivers of your neighbourhood?

15. *The Capercailzie*—The Capercailzie, or cock of the woods, existed formerly both in Ireland and Scotland; and, according to Shaw, one was killed in the latter country, about fifty years ago, at Lochlomond. It is much to be regretted that so magnificent a bird should have been lost; and it would be well worth any attempt to recover the breed. In Scotland there would be little doubt of its succeeding, if it could but be procured in sufficient numbers to make the attempt. The cock of the woods is by no means a difficult bird to rear, even in a state of captivity. There are several instances of its being kept alive in Sweden; and but very recently Captain

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* The Herling seems to be the *Salmo Albus* of Dr Fleming's "*British Animals*," and most ichthyologists—the species has not been thoroughly investigated.

Brooke was informed of two, where the female was sitting on several eggs, the result of which he was not acquainted with. All that it requires in its natural state is a considerable tract of wild country, well wooded with the fir, which may be considered necessary to the bird, as on its shoots it principally subsists during winter. If there be also a wide extent of mountains and high lands, it will be more favourable; and should the cranberry, the whortle or blackberry, and the other wild fruits which these situations produce, be found in abundance, the trial would, in all probability, be attended with success. In every part of Sweden they are found in abundance, as also in the southern parts of Norway. The soil, generally speaking, in both countries, is of a light and sandy nature; the forests almost wholly composed of fir, generally with little underwood; and the earth covered with the different kind of berries just noticed. What brushwood there is, is frequently the juniper and low birch, the berries of the former being also a favourite food of this bird. No attempt, Captain Brooke thinks, would ever succeed to rear them in this country by bringing their eggs over. Without speaking of other objections and impediments, the difficulty of meeting with the eggs would be sufficient. The peasants even seem to consider this as in a manner proverbial; and Captain Brooke never met with any one of them who had either seen the eggs or discovered a nest. The way in which they take the birds is principally by means of the gun, though sometimes snares are used. The offer of a good price is all that would be necessary; and with this temptation, there would be little fear of any insuperable difficulty. The old ones alone should be brought over, or birds of sufficient age to cause no apprehension in this respect. All the attempts that have been made by transporting young birds have uniformly failed from their dying shortly afterwards, whereas the old ones have lived. The female bird, during the period of incubation, is extremely shy, readily forsaking her nest when disturbed. In general, she lays as many as ten and twelve eggs, which are nearly equal in size to those of a hen. The ground of them is tawny white, but thickly covered with small blotches of a reddish brown, a few specks being some shades deeper, and approaching to black.—When the young birds are hatched they resemble the mother, and remain so till autumn, when the black plumage of the male begins to appear.—*Vide Captain Brooke's Travels in Lapland.*

16. *The Portuguese Man of War.*—A recently published number of the *North American Review* contains a description of a natural object but little known to common readers, though said to be familiar to those who navigate between the tropics, the beautiful and enigmatical insect commonly called the Portuguese Man of War. It is from a Memoir to Dr Tilesius, who accompanied Mr de Krusenstern in his voyage round the world. "This singular animal had several times been delineated, described, and endowed with names; yet not only its denominations were various, but also the nature and characteristics ascribed to it. According to some it was a polypus, according to others a zoophyte, and others ranged it among the Mollusca. Naturalists who followed in the steps of Linnæus have called it the Physalis. Wonderful as are all

the works of Providence, admirably fitted as are the several parts of each created being for their several functions, complex in their composition as they sometimes at first seem, while yet they are always found to be really so simple and suitable in their action on a nearer investigation, we may nevertheless venture to rank this little animated creature among the most curious phenomena. A worm between six and eight inches in length, which is found but in certain latitudes, has seemingly the skill and knowledge of an experienced navigator, and is in itself a little ship. Its evolutions are according to the winds; it raises and lowers its sail, which is a membrane provided with elevating and depressing organs. When filled with air, it is so light, that it swims on the surface of alcohol, and is at the same time provided with a structure, which furnishes it with the necessary ballast. When high winds would endanger its existence, it descends into the deep, and is never seen on the surface of the water. From the under side of the body proceed tubes, which extend twenty feet in length, and are so elastic and delicate, that they wind in a spiral form like a screw, serving at once as anchors, defensive and offensive weapons, pneumatic tubes and feelers. The insect has the colours of the rainbow; its crest, which performs the office of a sail, is intersected with pink and blue veins, trimmed with a rosy border, and swells with the winds, or at the animal's pleasure. The fibres contain a viscous matter, which has the property of stinging like nettles and produces pustules. It acts so strongly that vessels in which they have been kept for a time, must be repeatedly washed before they can be used. These fibres may be cut off without depriving them or the rest of the insect of the principle of life; and the separation takes place spontaneously, whenever the glutinous matter comes in contact with a hard surface like the sides of a glass globe. The insect has, however, dangerous enemies in small dolphins, and medusæ, against which neither its nautical skill nor its poison can defend it."

IV. GENERAL SCIENCE.

17. *Burning Coal Mine at New Sauchie.*—It is now more than two years since the snow lying on a field on the farm of Shaw Park, belonging to the Earl of Mansfield, was observed to melt almost as soon as it fell, and then rise in a state of vapour. The phenomenon attracted the attention of the managers of the Alloa and Devon collieries, and was found to be the effect of the heat produced by a stratum of coal in a state of ignition, technically known by the name of the nine feet seam, from which the Devon iron works are supplied with a large proportion of their fuel. Various plans were at the same time suggested to extinguish the flames, and after several failures, it was determined to cut a mine round the seam to prevent their extension. Workmen were set to excavate this mine, which was opened at both sides of the seam, to build a wall as they proceeded, on the sides of the two tunnels next the fire. In this way it was intended to proceed, till the tunnels penetrated beyond the fire, when they were to be joined in the form of a horse shoe, and thus cut off, by means of a strong wall, all connection between the ignited part of the seam and the remainder of it. This plan has been persevered in for a year and a half, but has

never been completed. The workmen have often brought the two walls within a few fathoms of meeting, but owing to the fire bursting in upon them, they have been hitherto obliged to fall back again and take a wider circle. Six or seven shafts have been sunk to ventilate the tunnels, in which the heat is frequently so great as to raise the thermometer from 212 to 230 degrees of Fahrenheit;—it sometimes rises even higher. The lamps of the miners which are hung upon the walls, have more than once fallen to pieces from extreme heat.—*Stirling Journal.*

18. *Hay converted into a Siliceous Glass by Lightning.*—In the summer of 1827, a rick of hay in the parish of Dun, near Montrose, was set on fire by lightning, and partly consumed. When the fire was extinguished by the exertions of the farm-servants who were on the spot, there was observed in the middle of the stack a cylindrical passage, as if cut out by a sharp instrument. This passage extended down the middle of the stack to the ground, and at the bottom of it there was found a quantity of vitrified matter, which there is every reason to think is the product of the silex contained in the hay which filled up the cylindrical passage. The existence of silex in the common grasses is well known, and the colour of the porous and vesicular mass is very like that which is obtained from the combustion of siliceous plants. We have been indebted for a specimen of the substance to Captain Thomson of Montrose, who examined the spot almost immediately after the accident had taken place.

19. *Application of Zinc to Roofing of Houses.*—It is perhaps not generally known that this metal rolled into large plates has been for some years adopted on the Continent as a substitute for lead and slates in the roofing of houses, and is now applied in this country to the same purpose in covering of public and private buildings. Roofs made of those plates are so light, that the timber requires not to be above half so strong as for lead or slates, there being only about *one-sixth* part of the pressure. Very handsome water cisterns and rain water pipes, &c. are also made out of zinc, specimens of which, we understand, may be seen at Mr JOHN CLARK's zinc warehouse, No. 38, George's Street. As an important saving is said to be obtained from the use of this metal, it appears particularly deserving of attention by those who are engaged in architectural improvements; and, as the ore exists in great abundance in this island, were it extensively used for the above purposes, a considerable consumption of one of the natural products of the country would be the consequence.

20. *M. Utenhove on Spherical and Parabolical Specula for Telescopes.*—In the new Memoirs of the first class of the Royal Institute of the Netherlands, vol. ii. part ii. lately published, there is an Optical memoir by M. Von Utenhove, in which he treats of the difference between spherical and parabolical specula employed for telescopes. His principal object is to determine what is the greatest error which is committed by substituting one of these conic sections for the other, and he has considered the question in a geometrical aspect. He afterwards applies his formula to the principal known telescopes, and he finds, that in the great telescope of Herschel, for

example, the greatest error, in substituting a spherical for a parabolical mirror, amounts to $\frac{1}{1778}$th of a line.

21. *Iron Trade of Great Britain.*—The whole iron made in Great Britain has been as follows:—

	Tons.	
1740,	17,000 from	59 furnaces.
1788,	68,000	121 furnaces.
1796,	125,000	
1806,	250,000	
1820,	400,000	
1827,	690,000	284 furnaces.

The iron produced in 1827 was made as follows:

	Tons.	
South Wales,	272,000	90 furnaces.
Staffordshire,	216,000	95
Shropshire,	78,000	31
Yorkshire,	43,000	24
Scotland,	36,500	18
North Wales,	24,000	12
Derbyshire,	20,500	14
	690,000	284

About 3-10ths of this is used for home consumption, and the other 7-10ths exported.—*Repertory* of Arts, October 1828.

22. *Explosion at the bottom of a Well at Bologna.* The well where this explosion took place is in the house of M. Berni Degli Curteni. As nothing preceded the explosion, the inhabitants of the house were greatly alarmed. The water of the well was analysed by Professor Sgarzi, who discovered in it only a little carbonic acid and salts, with a calcareous basis. M. Criole ascribes the explosion to the vapours of the water, and to the gas arising from the sulphuret of iron, with which the water has come in contact by infiltration, and to a winter excessively wet. The Fumaroli of Naples and the Grotto del Cane are analogous phenomena, and the Professor might have added also the slight commotions which the ground frequently experiences in the neighbourhood of the thermal springs of the Pyrenees, the heat of which is kept up, as is well known, by the constant decomposition of the sulphurets of iron.—*Rev. Encyclopédique.*

23. *Destruction of Live Stock by Wolves in Russia.*—In the government of Livonia alone, the following animals were destroyed by wolves in 1823. The account is an official one.

Horses,	1,841	Goats,	2,545
Fowls,	1,243	Kids,	183
Horned Cattle,	1,807	Swine,	4,190
Calves,	733	Sucking pigs,	312
Sheep,	15,182	Dogs,	703
Lambs,	726	Geese,	673

24. *Supposed Series of Sub-marine Banks from Newfoundland to the English Channel.*—From the Great Bank of Newfoundland to the English channel, it was found that whenever we approached towards the *Vigias*, or dangers laid down in the charts, the water changed from the deep blue of the ocean to green; in some instances to a light pea green; and this colour was not the effect of any change in the state of the atmosphere, but remained the same under the different alterations of sunshine, cloudy weather, and haze. These changes were so remarkable, that they became the subject of conversation on board, and occupied my attention particularly. On an inspection of the chart, I came to the conclusion that, as this part of the north Atlantic, lying between Newfoundland and the English channel, crosses the meridian of the volcanic islands of Iceland and the Azores, there are connecting ramifications between the subterranean fires of Iceland and those of St Michael of the Azores, and that the spaces of green water * over which we sailed in this route, were indications of the superior elevation of the bottom of the ocean in the lines of communication between the two volcanic lands above-named; and the coincidence of the water changing colour as we approached the different rocks, shoals and islets, placed in the chart in this part of the Atlantic, (some of which have been verified) supported the probability of the conclusion I had drawn. Assuming, therefore, that these banks, (which I conceive to be detached, that is to say, having deep water between them from N. to S.) exist, and are the lines or conductors of volcanic matter from Ireland to the Azores; we may readily account for the appearance and disappearance of such islands, rocks, &c. as Buss Island, the rocks seen by Sir Charles Knowles, those looked for by Admiral Rodney, westward of Ireland, Jaquett Island, the Devil's Rock, and the Eight Stones north of the Madeiras, &c. &c. because we have undoubted proofs that sub-marine volcanoes throw up islands and rocks from a very great depth, as in the instance of *Sabrina* island off St Michael's; and that islands disappear from the same cause, as instanced in the submersion of *Gouberman's* islands on the coast of Iceland, and *Rober's* isle at the Cape of Good Hope. I consider, therefore, that from the longitude of 10° W. to the Banks of Newfoundland, and from the Madeiras to Iceland, that is from 32° N. to 65° N. the ocean comprised within that area, is the seat of the different branches of sub-marine volcanic matter in the north; and this may account for the frequent shocks of earthquakes felt in Great Britain and Portugal. As far as my own ideas go concerning volcanoes, I am willing to believe, that throughout the whole earth they are connected by subterranean and sub-marine tubes or channels, and this hypothesis is borne out by facts so plain, as to be almost demonstrative with regard to earthquakes, which philosophers consider as

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* It may be worthy of notice, as a circumstance strengthening my opinion, that the Medusæ, Polypi, &c. were infinitely more abundant in these spaces of green water, than in those of a blue colour; indeed, very few of the larger species of these animals were seen in the latter, they were generally of the small orbicular kind; whereas in the green water they were frequently from three to five feet diameter, of an infinite variety of shapes and of the most brilliant colours.

occasioned by subterranean fire and water creating an exploding gaseous fluid. Upon this view of the subject, we might carry our line from the Madeiras to the Canaries, proceeding on to the Cape Verds, St Helena,* &c. &c. and it has often struck me, with respect to the Atlantide Island of the Ancients, if such ever existed, that it occupied that space of the ocean lying between Porto Santo and the Azores, and that these islands formed the extremes, the centre part having sunk into the bosom of the deep by the agency of volcanic fire. I may close these remarks by observing, that the captain (an officer of the navy, possessing experience and scientific knowledge) of the vessel in which I was, appeared at first sceptical with respect to my hypothesis, but at last, from his own attentive observations, became fully convinced of its probability.—*United Service Journal,* No. 14, 1830.

25. *New Islands on the Coast of Japan.*—Captain Coffin of the Nantucket has discovered six new islands situated near the coast of Japan, from which they are distant only four days sailing. They form a group situated to the south of Sandown Point. The Bay in which Captain Coffin cast anchor is in 26° 30′ of north latitude, and 141° of east longitude from Greenwich.—*Communication from Mr Warden to the Academy of Scinces, January* 1828.

26. *Agitation of the Sea in the Channel, and Earthquakes in Italy.*—During the eight or ten days which preceded the 17th of April 1828, an extraordinary motion of the sea had been felt in the channel, insomuch, that several outward bound vessels could not proceed to the westward of the Lizard; and the rise of the tide had been great for several days, viz.—nineteen feet, which produced a grand and terrific burst upon the Breakwater, many feet above the crane-head. We then were of opinion, that the extraordinary circumstance alluded to was caused by some convulsion of nature in some other quarter, and the information recently received proves that our opinion was correct. It appears that on the 10th of April, and subsequent days, shocks of earthquake were felt at Rome, Florence, and other parts of Italy. Now, as the extraordinary motion in the channel, and the shocks in Italy were felt at the same time, is it not fair to presume that the one was caused by or had connection with the other?—*Plymouth Journal.*

27. *Great Rain in Perth on the 3d August* 1829.—In a shower which seems to have been confined to the immediate vicinity of Perth, no less than *four-fifths* of an inch of rain fell in the course of half an hour. On the Oroonoko Humboldt observed 1½ inch of rain fell in three hours.—*Perth Courier,* 7th August.

28. *Earthquake at Bogota on the* 17*th June.*—We are in a state of great excitement and anxiety. Last night was the most awful one I ever passed.

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* All those islands are of volcanic origin.

We were sitting at whist as the clocks chimed a quarter to eleven; at that moment we were all sensible of the shock of an earthquake, and we pursued our game. About two minutes elapsed, when we experienced a most awful repetition. The walls of the house were dreadfully agitated, our candles were overturned, chairs and tables thrown from one side of the room to the other—we could ourselves scarcely maintain our erect positions, and were so perfectly paralyzed, that we never thought of getting out of the house; indeed, my own belief was, that the house must fall before we could possibly escape. The ceiling was coming down upon us in large flakes, and the fall of a large mirror at the moment, which we took to be part of the house, added to the alarm. It lasted forty seconds. We then went into the street, where crowds were on their knees praying most fervently. A general rush was made for the square in which the palace is. There we found thousands collecting and collected; women and men just as they had jumped out of bed, with the addition of a blanket thrown around them—mothers in the agony of grief and apprehension, clasping their children to their bosoms—fathers and brothers endeavouring to provide them with covering—groups of females in every direction calling each other's names to be assured that all was safe. Dismay and despair were general. No one would return home, and thousands passed the whole night in the square.

Three o'clock, P. M.—I have just returned from making a round of the town, to observe the extent of damage. Several houses are thrown entirely down; many are rent asunder from top to bottom. The cathedral, a splendid edifice, has one of its wings rent from the base to the tower. Scarcely a house in the city is without injury—mine has every one of its principal walls split in several places; the dining-room is in ruins; the partition of my bed-room has fallen in: and had I been in bed I should have been at least severely bruised. A severe shock has not been felt here until now, since the year 1805. About six years ago, it is said, there was a slight one, but no injury was done. It appears miraculous that only three lives have been lost. Many who are here, and were at Caraccas, during the great earthquake there, say, that this shock was much more severe; but the houses being better built here, the injury has been less.

Half-past five o'clock.—I have been taking another survey, and was surprised to find that hundreds of families are sending beds and bedding into the plain, and erecting booths there for the night.—All fear another shock.

19*th*, Twelve o'clock, noon.—This night has passed quietly, and the alarm is subsiding.—*Letter from Bogota.*

29. *Earthquake in the Netherlands, 23d February* 1828.—The number of earthquakes which are on record as having been experienced in the Netherlands for many centuries past, does not exceed six or eight; and none of them have been productive of disastrous effects. Within a space of ten years, during the last century, three only took place, one of which happened in 1755, immediately after the great earthquake at Lisbon; and the last was in 1760. The one which has lately occurred was particularly

felt along the banks of the Meuse; and its greatest violence was felt in the towns of Liège, Tongres, Tirelemont, and Huy: many of the walls and buildings of which suffered considerable injury; but happily no lives were lost. In the adjacent towns of Maestricht, Namur, Louvain, and Brussels, strong shocks were also experienced; but their violence diminished in proportion to the distance from the former or principal seat of concussion. They appear also to have been sensibly felt at Bonn, Dusseldorf, and Dordrecht, on one side, and at Flushing, Middleburg, and Dunkirk, on the other; although they were not perceptible at many of the intermediate towns. Slight shocks were also experienced at several of the frontier towns of France, as Avesnes, Commercy, and Longuyon; as also at the coal-mines near Liège, at the depth of from fifty to sixty toises; in which latter case they were accompanied by a hollow sound, resembling that of a heavy laden waggon. The direction in which the shocks were propagated appears to have been from east to west.

For some time before the earthquake the weather had been fine; but it became cloudy on the evening which preceded it, and continued so for several subsequent days. At Brussels the barometer had fallen during the three preceding days from 29.421 inches to 29.044; on the night before the earthquake it had risen to 29.126; and a few moments after the event, it stood at 29.233. It continued afterwards to rise; and on the 27th it had reached 30.166. At Liège, however, the barometer remained very low after the earthquake.

The shocks lasted about eight or ten seconds.

There have been experienced, since the 23d of February, slighter shocks; these also were preceded by a great depression of the barometer.—*Phil. Mag. July* 1828.

30. *Prizes.*—The President and Council of the Royal Society of London have adjudged the first royal medal to Charles Bell, Esq. for his researches relating to the nervous system, and the second to M. Mitscherlich for his discoveries relating to the laws of crystallization, and the properties of crystals.

Art. XXIV.—LIST OF PATENTS GRANTED IN SCOTLAND SINCE SEPTEMBER 23, 1829.

26. September 23. For certain Improvements on or additions to Fire Places. To Joseph Ange Fonzi, Esq. county of Middlesex.

27. September 23. For certain Improvements in the Construction of Cannon. To John Tucker, county of Middlesex.

28. September 23. For certain Improvements in Apparatus to be Applied to Fowling Pieces and other Fire Arms in place of Locks. To David Laurence and John Crundwell, county of Kent.

29. September 25. For a New Process or Method of Whitening Sugars. To Joshua Bates, city of London.

30. September 25. For an Improved Method of Constructing Steam-

Boilers or Generators, whereby the bulk of the boiler or generator and the consumption of fuel are considerably reduced. To JOSHUA BATES, city of London.

31. October 28. For certain Improvements in Diminishing Friction in Wheeled Carriages to be used on Rail-Roads, and which Improvements are Applicable to other Purposes. To ROSS WINANS, county of Sussex.

32. October 28. For a certain Improvement or Improvements in Distillation. To WILLIAM SHAND, Esq. county of Kincardine.

33. November 3. For certain Improvements in the Construction of Anchors. To WILLIAM RODGER, county of Middlesex.

34. November 6. For certain Improvements in the Process of Manufacturing Soap. To CHARLES TURNER STURTEVANT, county of Middlesex.

35. December 17. For certain Improvements in Machinery for Spinning Cotton and other fibrous substances. To CHARLES BROOK, county of York.

36. December 17. For a New Preparation or Manufacture of a certain material produced from a vegetable substance, and the application thereof to the purposes of affording light, and for other uses. To JAMES SOAMES Junior, county of Middlesex.

1. January 25, 1830. For an Exploding Shot or Projectile. To JOHN TUCKER, county of Middlesex.

2. February 2. For a New Alloy or Compound Metal applicable to the Sheathing of Ships and various other useful purposes. To JOHN REVERE, New York.

3. February 2. For a Machine or Hydraulic Engine for applying the power or pressure of Water, Steam, or other elastic fluids to the purpose of working Machinery and other uses requiring power, and applicable to that of raising or forcing fluids. To EDWARD DAKEYNE and JAMES DAKEYNE, county of Derby.

4. February 8. For an Improvement in Ships' Windlasses. To GEORGE STRAKER, county of Durham.

5. February 9. For an Improvement in the Manufacture of Canvas and Sail-Cloth for the Making of Sails. To JAMES RAMSAY and ANDREW RAMSAY, Greenock.

6. February 13. For an Improved Mechanical Power Applicable to Machinery of different descriptions. To THOMAS JOHN FULLER, county of Middlesex.

7. February 16. For certain Improvements on, or additions to, Wheels or Apparatus for Propelling Vessels and other purposes. To AUTON BERNHARD, county of Middlesex.

8. February 19. For an Improved Method of Manufacturing Salt. To JOHN BRAITHWAITE and JOHN ERICSSON, London.

9. February 26. For an Improvement in the Apparatus used for Distilling. To PATRICK DAWSON, Lillyburne.

10. February 26. For certain Improvements in Apparatus used for Distilling and Rectifying. To ROBERT BUSK, county of York.

11. March 3. For the manufacture or preparation of certain substances

which he denominates the British Tapioca, and the Cakes and the Flour to be made from the same. To JOHN M'INNES, Esq. of Stirling.

12. March 16. For certain Improvements in the Construction of Window-Frames, Sashes, or Casements, Sun-blinds, Shutters, and Doors, designed to afford security against Burglars, as well as to exclude the weather. To ANDREW SMITH, county of Middlesex.

13. March 16. For certain Improvements in Apparatus and Machinery for Cleansing and Deepening Rivers, and in the method of applying the same. To THOMAS AFFLECK, Dumfries.

14. April 13. For certain Improvements in Machinery for Spinning Cotton, Silk, Linen, and other Fibrous Substances. To JAMES CARRICK, Esq. county of Lancaster.

15. April 29. For certain Improvements in Making or Manufacturing Bolts or Chains. To SAMUEL BROWN, Esq. London.

16. April 29. For certain Improvements in the Means of Keeping or Preserving Beer, Ale, and other Fermenting Liquors. To WILLIAM AITKEN, Esq. Scotland.

17. May 3. For certain Improvements in Apparatus for Making and Supplying Coal Gas for useful purposes. To RICHARD WITTY, county of Stafford.

18. May 12. For certain Improvements on Steam Boilers and in Carriages or Apparatus connected therewith. To JAMES VINEY, Piccadilly.

19. June 14. For a New Method of Purifying and Whitening Sugar or other Saccharine Matter. To EDWARD TURNER, county of Middlesex.

20. July 21. For an Improved Engine for Communicating Power for Mechanical Purposes. To JOHN ERICSSON, London.

21. July 29. For certain Improvements in Preparing or Finishing Piece Goods made from Wool, Silk, or other fibrous substances. To JOHN FREDERICK SMITH, Esq. county of Derby.

22. July 29. For certain Improvements on Steam Carriages and in Boilers, and a Method of producing increased Draft. To JOHN RAWE JUNIOR, county of Middlesex.

23. August 3. For an Improvement or Improvements in the Method or Apparatus for Separating the Knots from Paper, Stuff, or Pulp, used in the Manufacture of Paper. To RICHARD IBOTSON, county of Middlesex.

24. August 19. For certain Improvements on, and Additions to, Machines or Machinery to be used and applied for conducting to and winding upon Spools, Bobbins, or Barrels, Rovings of Cotton, Flax, Wool, or other fibrous substances of the like nature. To JOSEPH CHESSEBOROUGH, county of Lancaster.

25. August 19. For a Method or Process of giving a Metallic surface to Cotton, Silk, Linen, and other fabrics. To JOHN YATES, county of Chester.

26. August 19. For a New Method of making Iron Wheel Barrows of wrought Iron, with a wrought Iron Wheel, by which New Method, said Iron Wheel Barrows can be made lighter, stronger, more durable, and cheaper than any iron wheel barrows which have been heretofore in use. To WILLIAM MALLET, Dublin.

27. September 6. For an Improved Machinery for the Navigation of Vessels and Propelling of Carriages. To John Ruthven, Edinburgh.

Art. XXV.—CELESTIAL PHENOMENA,

From October 1st to December 31st, 1830. Adapted to the Meridian of Greenwich, Apparent Time, excepting the Eclipses of Jupiter's Satellites, which are given in Mean Time.

N. B.—The day begins at noon, and the conjunctions of the Moon and Stars are given in Right Ascension.

OCTOBER.

D.	H.	M.	S.	
1	19	57		○ Full Moon.
2	11	15		♃ □ ☉
5	11	41	31	☽ ☌ γ ♉ ☽ 35′ N.
5	18	7	53	☽ ☌ α ♉ ☽ 17′ N.
6	8			♀ ☌ β ♍
8	10	32		☾ Last Quarter.
10	1			☿ ☌ ι ♍
10	7	6	3	Em. I. Sat. ♃
10	10			☿ ☌ α ♍
12	12	45		☿ Inf. ☌ ☉
14	19			☽ ☌ ♀
16				♅ Stationary.
16	7	31		● New Moon.
19				♂ Stationary.
21				☿ Stationary.
23	10			☉ enters ♍
24	10	20		☽ First Quarter.
28				☿ Greatest Elong.
28	4			☽ ☌ ♀
29	10			♃ ☌ o ♂
30	7	1	55	☽ ☌ ν ♓ ☽ 72′ N.
31	8	39	2	☽ ☿ μ Ceti ☽ 71′ N.
31	5	18		○ Full Moon.

NOVEMBER.

D.	H.	M.	S.	
2	3	59	13	☽ ☌ α ♉ ☽ 25′ N.
6	22	53		☾ Last Quarter.
7	10			♀ ☌ λ ♍
10	4			☿ ☌ λ ♍
15	1	55		● New Moon.
15	6	8	14	Im. IV. Sat. ♃
15	7			☿ ☌ 2 α ♎
18	5	40	3	Em. I. Sat. ♃
21	14			♀ ☌ ☿
22	6	37		☉ enters ♂
22	23	44		☽ First Quarter.
23	5	19	7	Em. II. Sat. ♃
23	21			♀ ☌ κ ♎
24	5	40	45	Im. III. Sat. ♃
26	2			♀ ☌ λ ♎
28	10			♀ ☌ 1 and 2 β ♍
29	9	3	9	☽ ☌ γ ♉ ☽ 46′ N.
29	15	13	51	☽ ☌ α ♉ ☽ 29′ N.
29	15	8		○ Full Moon.
29	16			♀ ☌ ν ♍

DECEMBER.

D.	H.	M.	S.	
5	7	26	53	☽ ☌ α ♌ ☽ 68′ S.
3	16			☿ Sup. ☌ ☉
6	15	16		☾ Last Quarter.
7	10	23	18	☽ ☌ β ♍ ☽ 66′ N.
12				♄ Stationary.
12	7	59	2	☽ ☌ γ ♎ ☽ 31′ N.
14	20	19		● New Moon.
20	16			♀ Sup. ☌ ☉
21	19	8		☉ enters ♑
22	10	42		☽ First Quarter.
25	4	49	39	☽ ☌ μ Ceti ☽ 57′ N.
27	1	36	35	☽ ☌ α ♉ ☽ 74′ S.
29	2	2		○ Full Moon.
29	2			♂ ☌ ε ♓

Times of the Planets passing the Meridian.

OCTOBER.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	h	′	h.	′	h	′	h	′	h	′	h	′
1	1	10	22	49	11	11	6	10	21	29	8	7
13	23	47	23	0	10	19	5	32	20	50	7	23
25	22	55	23	10	9	33	4	53	20	9	6	38

NOVEMBER.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
1	22	56	23	16	9	8	4	31	19	43	6	11
13	23	17	23	25	8	29	3	52	18	58	5	24
25	23	42	23	36	7	53	3	12	18	10	4	36

DECEMBER.

D.	h	′	h	′	h	′	h	′	h	′	h	′
1	23	56	23	41	7	36	2	51	17	45	4	11
13	0	22	23	53	7	3	2	10	16	53	3	21
25	0	53	0	5	6	31	1	28	16	59	2	30

Declination of the Planets

OCTOBER.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	°	′	°	′	°	′	°	′	°	′	°	
1	14	10 S.	6	8N.	6	51 S.	23	25 S.	13	29N.	19	16 S.
13	8	38	0	20N.	6	43	23	21	13	6	19	17
25	3	44	5	35 S.	6	52	23	14	12	46	19	17

NOVEMBER.

1	6	6 S.	8	56 S.	5	5 S.	23	85	12	35N.	19	15 S.
13	13	12	14	17	3	20	22	56	12	22	19	11
25	19	45	18	45	1	10	22	40	12	13	19	7

DECEMBER.

1	22	14 S.	20	33 S.	0	1N.	22	31 S.	12	11N.	19	3 S.
13	25	9	23	4	2	33	22	10	12	11	18	56
25	24	45	23	54	5	14	21	44	12	17	18	46

Art. XXVI.—*Summary of Meteorological Observations made at Kendal in June, July, and August* 1830. By Mr Samuel Marshall. Communicated by the Author.

State of the Barometer, Thermometer, &c. in Kendal for June 1830.

Barometer.	Inches.
Maximum on the 9th, - - -	30.06
Minimum on the 4th, - - -	29.34
Mean height, - - - -	29.65
Thermometer.	
Maximum on the 27th, - - -	65.5°
Minimum on the 5th, - - -	39°
Mean height, - - - -	52.20°

Quantity of rain, 5.289 inches.
Number of rainy days, 21.
Prevalent winds, north and west.

This has been a very rainy and unsettled month, and but little progress made in the hay harvest in consequence. The showers have been seldom heavy, but of long continuance, and they have been mostly of a drizzling kind. The weather has been generally cold for the season, as the mean indicates, and far from genial. The barometer has been mostly about a mean height. The winds from the north have been much more prevalent than is common in this month.

July.

Barometer.	Inches.
Maximum on the 28th,	30.23
Minimum on the 9th,	29.15
Mean height,	29.74
Thermometer.	
Maximum on the 31st,	81°
Minimum on the 11th,	42°
Mean height,	58.59°

Quantity of rain, 4.961 inches.
Number of rainy days, 19.
Prevalent wind, west.

Till the 26th, there were but seven days on which a greater or less quantity of rain had not fallen. The last week in the month was very seasonable, and for a few days the weather was hot. On the 30th we had a tremendous storm of thunder and lightning, which extended to several counties round this, and in most places was very severe: though this town appears to have been near the centre of its extent. The wind was in the west 14, and in the S. W. 11 days in this month.

August.

Barometer.	Inches.
Maximum on the 19th,	30.05
Minimum on the 28th,	29.08
Mean height,	29.66
Thermometer.	
Maximum on the 15th,	63.5°
Minimum on the 18th,	39
Mean height,	54.64°

Quantity of rain, 4.218 inches.
Number of rainy days, 19.
Prevalent wind, west.

Though we have had no great weight of rain during this month, yet the prevalence of showers has been so great, that we have seldom had an interval of more than a day at a time of fine weather. Though the evenings have begun to feel sufficiently chill to remind us of the approach of Autumn, yet the temperature through the days has been remarkably uniform, the highest that the thermometer has reached being 63½, and the lowest 56°, in the day time; and during the nights, the variation has been between 56° and 39°: of course we have had no frost in this part of the valley, though in the high land in the neighbourhood there has been a hoar frost occasionally. The total amount of rain for the year is 33.203 inches,—a much smaller quantity than might have been expected from the number of rainy days, for in the last five months, out of the 153 days in that period, there have been 94 on which rain has fallen. The aurora borealis was very brilliant on the evening of the 20th,—a phenomenon seldom seen at this season of the year.

ART. XXVII.—REGISTER OF THE BAROMETER, THERMOMETER, AND RAIN-GAGE, *kept at Canaan Cottage.* By ALEX. ADIE, Esq. F. R. S. Edin.

THE Observations contained in the following Register were made at Canaan Cottage, the residence of Mr Adie, by means of very nice instruments, constructed by himself. Canaan Cottage is situated about 1¼ mile to the south of Edinburgh Castle, about 3 miles from the sea at Leith, and about ¼ of a mile N. of the west end of Blackford Hill. The ridge of Braid Hills is about 1 mile to the south, and the Pentland Hills about 4 miles to the west of south. The height of the instruments is 300 feet above high water-mark at Leith. The morning and evening observations were made about 10 A.M. and 10 P.M.

JUNE 1830.

D. of Week.	Day of Month.	Thermometer.			Register Therm.			Barometer.		Rain.
		Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.	
T.	1	53	47	50	43	59	51	29.35	29.62	
W.	2	57	47	52	45	61	53	29.80	29.76	
T.	3	50	48	49	43	51	47	29.50	29.41	.24
F.	4	48	47	47.5	46	61	53.5	29.41	29.61	
S.	5	57	48	52.5	40	62	51	29.65	29.71	
S.	6	58	49	53.5	45	60	52.5	29.65	29.62	
M.	7	55	44	49.5	47	58	52.5	29.68	29.86	.13
T.	8	52	49	50.5	43	55	49	29.98	30.10	
W.	9	52	48	50	44	56	50	30.10	30.06	
T.	10	56	52	54	46	66	56	29.87	29.70	
F.	11	62	53	57.5	43	66	54.5	29.59	29.36	.06
S.	12	57	47	52	49	61	55	29.33	29.43	
S.	13	56	49	52.5	43	59	51	29.30	29.32	.09
M.	14	58	46	52	45	61	53	29.37	29.53	.19
T.	15	52	49	50.5	40	54	47	29.50	29.56	.21
W.	16	55	44	49.5	40	59	49.5	29.73	29.80	
T.	17	58	53	55.5	39	67	53	29.73	29.57	.22
F.	18	55	42	48.5	48	57	52.5	29.29	29.28	.67
S.	19	52	43	47.5	37	55	46	29.25	29.23	.20
S.	20	56	50	53	40	59	49.5	29.28	29.40	
M.	21	52	46	49	43	55	49	29.38	29.34	.04
T.	22	52	46	49	42	55	48.5	29.36	29.53	
W.	23	55	48	51.5	35	66	50.5	29.63	29.68	
T.	24	54	47	50.5	45	56	50.5	29.73	29.74	
F.	25	52	47	49.5	46	57	51.5	29.73	29.60	.31
S.	26	50	54	52	45	64	54.5	29.43	29.52	
S.	27	60	58	59	47	62	54.5	29.57	29.55	.18
M.	28	65	55	60	49	69	59	29.43	29.42	
T.	29	61	50	55.5	51	64	57.5	29.48	29.71	
W.	30	61	55	58	45	70	57.5	29.81	29.87	
	Sum,	1661	1461	1561	1314	1805	1559.5	886.91	887.89	2.54
	Mean.	55.37	48.70	52.03	43.80	60.17	51.98	29.563	29.596	

JULY 1830.

D. of Week.	D. of Mon.	Thermometer.			Register Therm.			Barometer.		Rain.
		Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.	
T.	1	63	51	57	47	68	57.5	29.73	29.61	
F.	2	58	52	55	48	58	53	29.46	29.39	.34
S.	3	53	50	51.5	50	57	53.5	29.43	29.48	.64
S.	4	58	48	53	48	64	56	29.59	29.64	.03
M.	5	59	53	56	48	61	54.5	29.80	29.78	.46
T.	6	56	56	56	49	61	55	29.67	29.40	.57
W.	7	59	52	55.5	52	55	53.5	29.27	29.28	.62
T.	8	58	50	54	47	58	52.5	29.24	29.03	.22
F.	9	53	52	52.5	41	61	51	29.13	29.35	.16
S.	10	59	57	58	44	66	55	29.50	29.62	.32
S.	11	57	57	57	40	60	50	29.60	29.46	.34
M.	12	52	45	48.5	47	54	50.5	29.33	29.59	.09
T.	13	59	52	55.5	44	62	53	29.81	29.86	
W.	14	60	59	59.5	48	62	55	29.67	29.45	.21
T.	15	62	55	58.5	55	67	61	29.44	29.53	
F.	16	61	54	57.5	58	63	60.5	29.46	29.64	.06
S.	17	60	58	59	48	66	57	29.43	29.21	.17
S.	18	60	51	55.5	52	63	57.5	29.30	29.48	.10
M.	19	61	55	58	50	68	59	29.65	29.70	
T.	20	62	57	59.5	51	64	57.5	29.63	29.70	
W.	21	63	60	61.5	51	63	57	29.79	29.87	
T.	22	66	62	64	56	72	64	29.85	29.65	.09
F.	23	62	54	58	56	65	60.5	29.73	29.70	.15
S.	24	64	58	61	47	66	56.5	29.78	29.81	
S.	25	70	68	69	61	74	67.5	29.87	29.93	
M.	26	71	64	67.5	61	75	68	29.95	29.07	
T.	27	75	62	68.5	58	77	67.5	30.15	30.18	
W.	28	76	62	69	52	81	66.5	30.25	30.20	
T.	29	67	59	63	51	70	60.5	30.17	30.15	
F.	30	64	61	62.5	54	66	60	29.73	29.57	1.80
S.	31	62	50	56	52	66	59	29.75	29.75	.20
		1910	1724	1817	1566	2013	1789.5	919.18	918.08	6.57
		61.61	55.61	58.61	50.51	64.93	57.72	29.651	29.616	

AUGUST 1830.

D. of Week.	D. of Mon.	Thermometer.			Register Therm.			Barometer.		Rain.
		Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.	
S.	1	57	58	57.5	47	64	55.5	29.58	29.33	
M.	2	62	54	58	53	65	59	29.23	29.25	.19
T.	3	65	55	60	54	68	61	29.53	29.72	
W.	4	62	55	58.5	54	65	59.5	29.76	29.61	.06
T.	5	60	52	56	49	60	54.5	29.63	29.70	.52
F.	6	58	52	55	47	59	53	29.73	29.68	
S.	7	60	48	54	48	62	55	29.65	29.58	
S.	8	60	52	56	42	64	53	29.57	29.58	.09
M.	9	58	53	55.5	48	60	54	29.63	29.58	
T.	10	58	53	55.5	46	60	53	29.55	29.53	.07
W.	11	58	53	55.5	49	60	54.5	29.38	29.13	.27
T.	12	61	54	57.5	49	65	57	29.06	29.28	.16
F.	13	65	53	59	50	64	57	29.28	29.37	
S.	14	60	53	56.5	49	63	56	29.40	29.33	.17
S.	15	61	51	56	48	63	55.5	29.45	29.44	1.51
M.	16	50	57	53.5	45	52	48.5	29.66	29.73	.86
T.	17	52	42	47	42	56	49	29.82	29.88	
W.	18	53	49	51	40	58	49	29.98	30.03	
T.	19	54	49	51.5	43	57	50	30.03	29.90	
F.	20	51	44	47.5	37	59	48	29.82	29.80	
S.	21	51	46	48.5	37	60	48.5	29.80	29.80	
S.	22	54	51	52.5	40	58	49	29.82	29.68	.17
M.	23	54	56	55	48	63	55.5	29.60	29.52	.10
T.	24	56	55	55.5	50	60	55	29.50	29.37	.37
W.	25	59	47	53	49	61	55	29.26	29.38	.43
T.	26	50	45	47.5	43	57	50	29.47	29.50	
F.	27	51	48	49.5	36	57	46.5	29.48	29.29	1.02
S.	28	49	46	47.5	43	49	46	29.16	29.42	.65
S.	29	53	47	50	40	55	47.5	29.66	29.80	.05
M.	30	54	45	49.5	44	57	50.5	29.89	29.90	
T.	31	57	62	59.5	41	54	47.5	29.90	29.90	
		1753	1585	1669	1411	1855	1633	917.28	917.01	6.69
		56.55	51.13	53.84	45.52	59.84	52.68	29.590	29.581	

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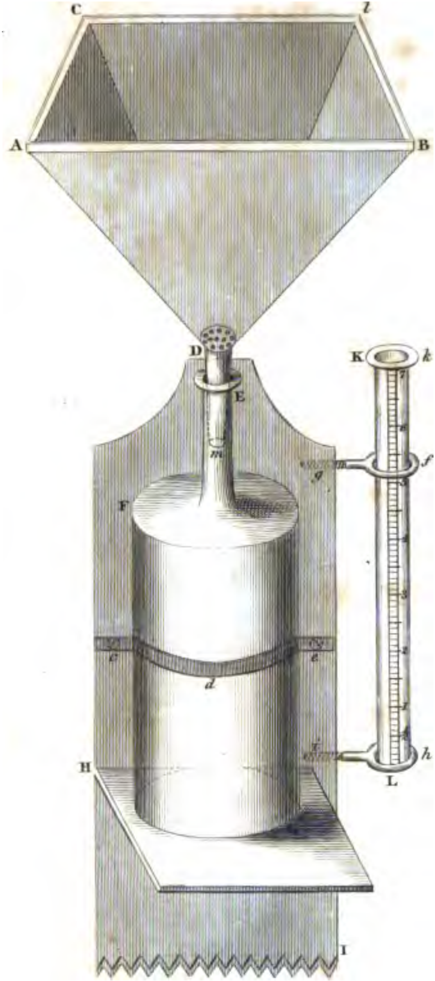

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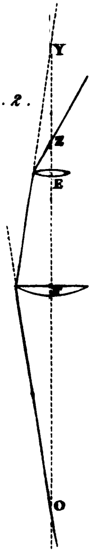

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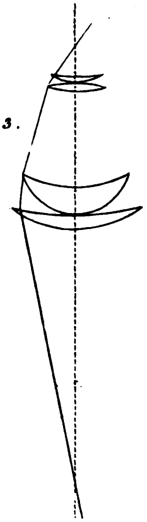

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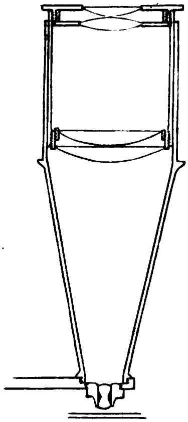

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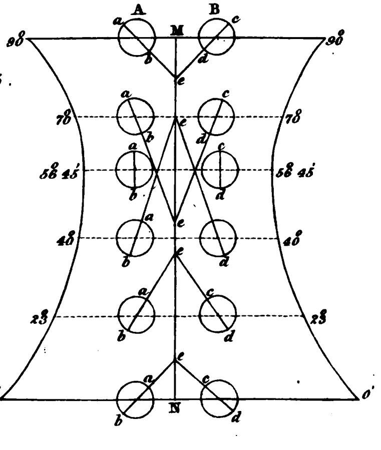

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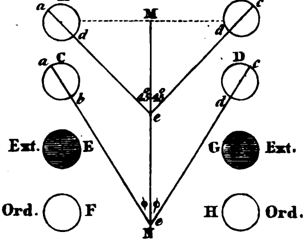

PLATE II.

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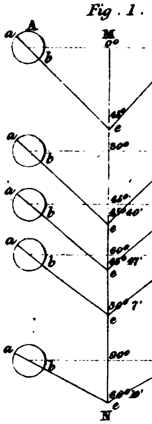

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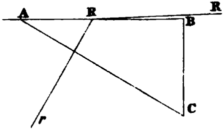

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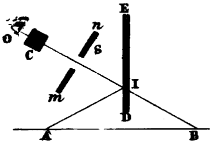

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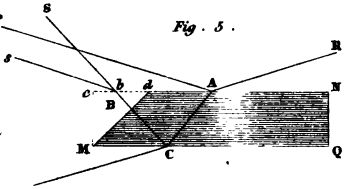

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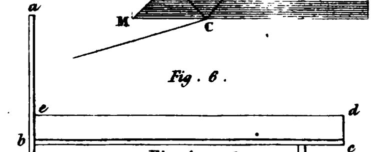

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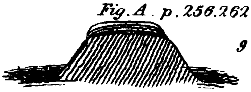

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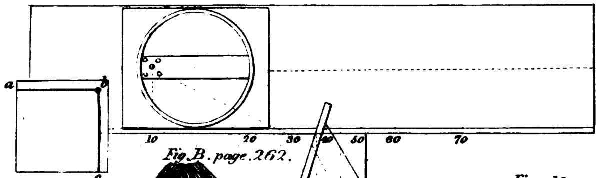

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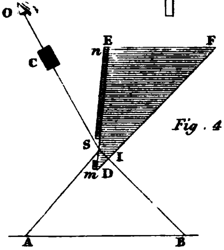

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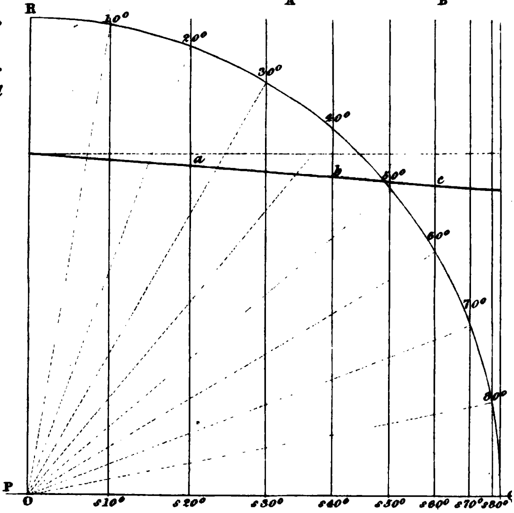

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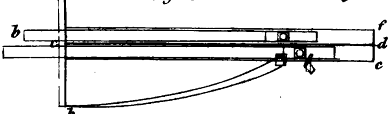

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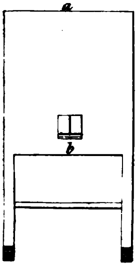

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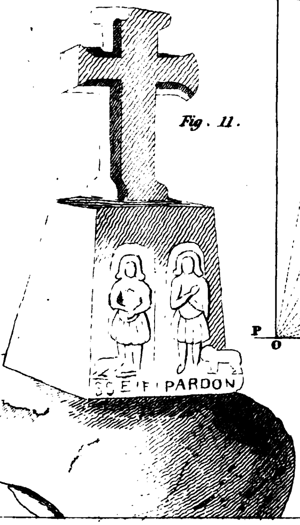

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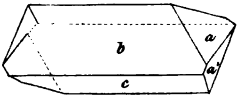

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Fig. 13.